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LECTURES

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BY

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EXTRACTION OF FOREIGN BODIES.

Foreign bodies in the nasal passages. In the external ear. In the digestive tube. Removal of fish-bones from the throat. Impaction of bodies in the œsophagus. Foreign bodies in the stomach. Knife swallowing, and injurious bodies in the intestines. General treatment.

GENTLEMEN,—Two or three years ago I was consulted concerning a young person, a female, who had some complaint in her nostrils. There was a putrid discharge from them, and those symptoms were present which usually indicate the presence of diseased or dead bone of the nostrils; and presuming that this was the nature of the case I prescribed sarsaparilla, and treated her accordingly. This complaint had been going on since she was quite a child, and when I saw her she was eleven or twelve years of age. Not long ago, in blowing her nose, something came out of her nostrils—a large solid substance. Her family thought that this was the piece of dead bone which was expected to appear, and it was sent to me; but, on examining it, I found that it was not bone, nor had it the appearance of ever having been organised. It was convex on one side and concave on the other, and seemed to have been formed upon a nucleus. Dr. Prout was good enough to examine it chemically, and he found it to consist of dry mucus, with phosphate of lime, such as is secreted by an inflamed mucous membrane. The mucous membrane of the nose, like that of the bladder, will, when irritated, secrete phosphate of lime. I was led, from this, to conclude that, originally, some foreign substance had been introduced into the nose, and if it were a round body this would account for the concavity on one side of the concretion. Here was a case in which there was great reason to believe that some foreign body had been introduced into the nostrils, and had remained there for years, producing all the symptoms usually arising from diseased bone.

A little boy was brought to me a few years ago, with a putrid discharge from the nostrils. There, also, I thought that there was a piece of diseased bone. He had had this for one or two years. On looking into the nostril, however, I perceived, at the upper part, something rather larger than a piece of dead bone might be supposed to present. I took hold of it with the forceps, and, on removing it, found it was a tamarind-stone which the boy had thrust into the nostrils a year or two before, no one knowing anything of it. In each of these cases, when the foreign body was taken away, the symptoms subsided.

Another patient was brought to me supposed to have diseased bone in the nose,—a little girl in whom there had been a putrid discharge for two or three years. There I could see nothing, but, from the symptoms, I concluded that disease was going on in the bone. I prescribed for this patient sarsaparilla, and one morning something was blown out of the nose. It was brought to me, and I discovered that it was a piece of sponge that had stuck in the nostril, and was now filled with mucus, and, I suppose, some phosphate of lime. As no one knew the history of the case I suppose that the child must have thrust it in herself. It is not very uncommon for children to get foreign bodies into their nostrils, and these cases show that you may be led into great error by supposing that there is diseased bone when there is none at all.

In two of these cases the foreign body was blown out—came away spontaneously; and in the case of the tamarind-stone I removed it very easily with a pair of forceps. Other means, however, may be adopted for removing these foreign bodies. A child was brought to me who had got a glass bead into the nostril, and it was known that it was there. I tried to take hold of it with the forceps but they slipped over its smooth sur-

face. I then introduced a probe, bent in a peculiar manner, which, getting behind the bead, pulled it out.

Foreign bodies may get into the external meatus of the ear. A child was brought to me who had got a broken piece of slate-pencil, about half an inch in length, in the meatus. You might suppose it an easy matter to get a foreign body out of the external meatus of the ear, that part being so much more in sight than the nostril. But it is often very difficult, and for this reason: in the nose you may poke with the forceps, and do no harm. I have already stated what great manipulations the nostril will bear. But what will happen if you poke with the forceps in the ear? A child was brought to this hospital with a pea in the ear. A great many attempts had been made to remove it prior to the child being brought here. The pea was then out of sight, and the child had very alarming symptoms of inflammation of the brain. The little patient died; and it was found that in attempting to extract the pea, the membrana tympani had been destroyed. The injudicious poking of the tympanum with the forceps had caused inflammation of the bone of the tympanum, and a separation between it and the dura mater, so that the child died in consequence of the rude introduction of the forceps into the ear. Indeed, it is a very difficult thing to extract a foreign body from the ear with forceps, and if you attempt it you must proceed with the greatest caution. I have, however, extricated foreign bodies from the ear with a narrow pair of forceps, by letting the rays of the sun shine into the meatus, and then introducing the forceps, so that one blade came upon each side of the foreign body. But if you attempt it without the rays of the sun shining into the ear, and using your eyes carefully, and your hands slowly and attentively, nothing is more easy than to drive the body against the membrana tympani, break the latter, and push the body into the tympanum. I do not say that you are not to extract foreign bodies from the ear with forceps, but you must do it with the greatest care; for the want of care may lead to the destruction of the patient. But I have more frequently succeeded in these cases by other means. I stated that a child was brought to me with a piece of slate-pencil in the ear. I placed the child opposite the light, and injected some tepid water into the ear with a syringe. There was room for the water to penetrate into the meatus, and as it came back it washed out the slate-pencil. There was a case brought into the hospital in which there was some foreign body—I believe a pea—in the external meatus. I tried all sorts of methods to get it out. I could not use the forceps, and it nearly filled up the meatus, so that either water could not pass behind it, or it was so jammed that the water injected by the syringe would not wash it out. I said, "Let it alone, let it remain there, the pea in all probability will dry and waste of itself, and then it will come out, or when it is rotten it may be washed away with a syringe; but I will not make any further efforts to remove it now; for I may drive it into the tympanum and kill the patient." In one case, where a foreign body had got into the ear, I extracted it, like the glass bead, with a bent probe, which I introduced very carefully behind it.

Having called your attention to this subject, I shall proceed to speak of foreign bodies in other cavities. You may find them in any cavities that have natural outlets. They may be thrust in, or they may be swallowed. They may, when swallowed, pass at once into the stomach; some, from their bulk or irregular figure, stick in the pharynx or œsophagus; and others, even of small size, if sharp and pointed, may stick in the pharynx or tonsils.

The small bones of fish, if they be swallowed, and stick anywhere, generally do so in the tonsils. The following is not a very uncommon case:—A patient sends for you who has swallowed a fish-bone; he feels an uneasy sensation, and every time he tries to swallow he finds pain, and the more he attempts to swallow the greater is the pain. You look into his throat and see a fish-bone sticking in the tonsil. Nothing can be more easy than to hold down the tongue with one finger on the flat end of a spoon, take hold of the fish-bone with a pair of forceps and remove it. The fish-bone, however, may be stuck in the lower part of the pharynx, and then you cannot see it; but you may feel it with the finger, and having so done you may seize it with the forceps and remove it. The part at which fish-bones most frequently stick is where the œsophagus and pharynx unite just behind the cartilages of the larynx. The reason why they are so liable to stick there is, that the cartilages of the larynx are not capable of being dilated; whereas, if they pass lower down, the whole tube of the œsophagus may become dilated.

The *treatment* of these cases differs much according to circumstances—according to the exact position of the body swallowed, and according to the nature of the body itself. A person swallows a large piece of meat, and it sticks somewhere in the pharynx or œsophagus. If, on introducing the finger, you feel it quite distinctly in the pharynx, there is no reason why you should not remove it with forceps. But if it lodge in the œsophagus, then the best thing that can be done is, to introduce a common œsophageal bougie and push the piece of meat down into the stomach. A little skill is necessary in introducing the bougie. There was an Indian juggler who used to swallow a large blade sword. The sword was straight, and he pushed it readily into the stomach. The way in which it was done was

this :—The man threw his head as far back as possible,—and, from early tuition, he could do that further than any of us,—so that he made the mouth, the pharynx, and the œsophagus, one straight line, and then he introduced the sword. You should act on this principle in introducing a bougie. Let the patient be placed on her chair, as it occurs more frequently in hysterical women than in others, with her head turned back as far as possible; and then having a bougie well oiled, introduce it to the pharynx, and with the finger push it down. If it meets with resistance, use moderate force to push the piece of meat into the stomach. A moderate force is always sufficient; you must be careful how you employ great force. I knew of a case where a surgeon, using a bougie roughly, pushed it through the œsophagus into the posterior mediastinum and killed the patient. I heard of another case where the same thing happened. However, it must require considerable force to push the bougie through the œsophagus; and it is only a moderate force that is necessary to push the meat into the stomach. But supposing it to be not a piece of meat, but a piece of bone, or any other foreign body; first ascertain whether it is within reach of the finger. I have already stated that a large piece of bone will generally stick in the lower part of the pharynx where that and the œsophagus unite, and you may then feel it with the finger. Endeavour to introduce the finger behind the glottis, and if you can do that, seize the bone with the forceps. You must be prepared with different kinds of forceps, some of which open laterally. It may be that the foreign body lies with two flat surfaces, one to one side and the other to the other, and then the forceps that open laterally answer best. If it be in the other position, with the flat surfaces looking forwards and backwards, you must have forceps which open in another direction. You may sometimes employ shorter forceps, and in other cases longer, but they should be of tolerable length.

But let us suppose that the foreign body cannot be felt with the finger, are you then to attempt to take hold of it with forceps. Really, to extract a foreign body from the œsophagus, below the part at which you can feel it with the finger, would be a very difficult operation, and probably not a very safe one; for, in poking with the forceps, you might carry them through the coats of the œsophagus. It might require great force to drive a bougie through them, but much force would not be required in order to drive through them a strong body made of steel. If the foreign body be low down, and you are to extract it at all, you must do it by other means; but probably it will be best to push it on into the stomach. If it be small enough to pass the œsophagus, it certainly will be small enough to pass the pylorus; at least, in all probability. You may push it into the stomach best by means of a common bougie, or what is called a probang—a piece of whalebone with a sponge at one end. This is to be introduced into the œsophagus and pushed down towards the stomach. It may operate in two ways. It generally acts by the sponge pushing the substance into the stomach; at other times, if the foreign body do not occupy the whole diameter, but only impinges by its two shoulders, the probang may be passed below it, and as you pull up the sponge the foreign body may be drawn up with it. You make a sort of blunt hook, to be fastened to the whalebone, the intention of which is that it should be passed below the foreign body, and the foreign body dragged up by the blunt hook. The best thing, however, that you can do is to push it into the stomach, and that is the most easily accomplished.

Although it is easy to speak of dislodging these foreign bodies, you will not always find it so easy in practice; and if you cannot easily remove them, what are you to do? If the patient suffer very little inconvenience, and the part be beyond the reach of the finger, I think it is best to let them alone; but if the part be within reach of the finger, then there can be no doubt as to the propriety of attempting to remove them. If, however, there be great difficulty in dislodging the body then it is best to let it alone, and nature will generally do what is wanted. The œsophagus will, by giving way, dilate below; the fibres will contract above; and gradually the thing will creep down to the stomach; or, perhaps, it may be hawked up again. I was called to a gentleman who said that he had swallowed a large piece of fish-bone—a part of the head of a cod. I could feel nothing with the finger; I passed a bougie into the stomach, and, to state the truth, I rather doubted whether anything had lodged there. As his life was not in danger, although he was suffering some inconvenience, I thought I would let it alone. In two or three days he hawked up something, and there came away a piece of bone, larger than the thumb, which had been lodged in the œsophagus. According to my experience, in the majority of cases where foreign bodies are stuck in the œsophagus, if you fail in relieving the patient, nature will accomplish it. I cannot say that I have seen any cases where any ultimate harm has arisen from a foreign body stuck in the œsophagus. Such cases have occurred, and there have been instances where a foreign body has pressed on the trachea and obstructed respiration, so that a patient has been nearly suffocated. If you are called to such a case the first thing you will do is, to make an opening into the trachea so as to enable the patient to breathe, and then you may examine the œsophagus and pharynx, and ascertain whether the foreign body can be removed or not. Cases

have been recorded where an incision has been made into the œsophagus for the purpose of taking out the foreign body lodged in it; and other cases are upon record where the foreign body has occasioned suppuration of the œsophagus and an abscess in the neck, and on opening it the foreign body was found in the cavity of the abscess. Such instances, however, are very rare; and on looking over the cases recorded in the Memoirs of the French Academy of Surgery, where there is a large collection presented, drawn from the authors of all ages, I do believe that, in the great majority, where the operation has been performed for the removal of foreign bodies from the œsophagus, the patients would have done much better if they had been left altogether to nature, and to the operation of their own powers.

Now, supposing the foreign body to have got into the stomach, what will it do there? Why, small bodies over and over again get into the stomach, and come away. If it be a sixpence, or a farthing, you may be pretty sure that in the course of two or three days it will be found in the evacuations. It is astonishing what foreign bodies will pass through the stomach, and go through the intestines, without doing harm. A gentleman, in a paroxysm of insanity, swallowed a pair of compasses three inches in length, and the family sent to me in great fright. The compasses had not stuck in the œsophagus, but had gone into the stomach. To think of looking for them there was quite absurd, and I told them to let him alone. He must have swallowed them with the blunt end forwards, and the probability was that they lay towards the pylorus. In the course of a fortnight, without his having suffered even a colicky pain, they one day found the compasses in his close-stool pan. He lived a considerable time afterwards, and never suffered any inconvenience from this exploit. Several persons have been in the habit of swallowing large bodies, and getting money for exhibiting the feat. A sailor, in America, in a drunken fit, swallowed a large knife. It went into the stomach, produced some colicky pains for a few days, and was then voided, per anum. Two or three days afterwards he did the same thing, and finding that people stared at him, and gave him money, he went on with it. People went on purpose to see this exhibition of swallowing knives. By and by, however, he got into very ill health; there was severe colicky pain in the intestines, and in the abdomen; his stools always came away black; and he sank, and died. On examining the body several blades of knives were found, half destroyed, from the oxidation to which they had been subjected. But it seemed that the immediate cause of death was a large knife which stuck across the upper part of the rectum, running through both sides of the gut.

The great majority, even of large substances, taken into the stomach, pass through the pylorus, travel along the intestines, and find their way out at the anus. There are particular parts of the intestines, however, where these foreign bodies are most likely to stick; they may remain in the cul-de-sac of the cæcum. A woman was brought here with a tumour in the right iliac region. She died, and, on examining the body after death, an abscess was found connected with the cæcum, and in the middle of the abscess there was a pin. Over and over again women and children swallow pins, and they generally pass away without doing harm, but in this case the pin stuck in the cæcum, and getting into the cellular membrane it caused a small infiltration of feculent matter and produced the abscess. The part, however, in which foreign bodies are most likely to remain, is the rectum. No doubt that abscesses by the side of the rectum and fistulæ in ano, in many instances, arise from some foreign body sticking in the rectum. I was called to a gentleman suffering great uneasiness in the rectum. At first I thought there were piles, but when he described his symptoms more accurately I was convinced that there was something more than internal piles. I introduced my finger into the rectum, and found that there was some hard substance above the sphincter, and which appeared to be half in the gut and half out. With some difficulty I dislodged it, seized it with a pair of forceps, and removed it. It turned out to be a large core of an apple, the sharp edge having stuck in the rectum. If it had not been thus removed it would have made an abscess. I was sent for to a gentleman with a large abscess by the side of the rectum. The patient had a dry, brown tongue, and other typhoid symptoms, and I therefore concluded that it was full of putrid matter. I opened the abscess freely, and let out a large quantity of stinking putrid matter. Having done that I thought it advisable to examine the abscess with my finger, and I found a hard body, sticking in it, like a great pin. With some difficulty I removed it, and it proved to be a fish-bone, perhaps two inches in length, one end of which had stuck in the side of the rectum, and the other lay across the abscess. He had swallowed it without being aware of it; it had passed easily down the œsophagus, through the stomach and pylorus, and all the coils of the intestines and cæcum, but when it reached the rectum it passed through one side of it, allowed some of the fecal matter to intrude by its side, and caused this large abscess. Many cases are recorded by writers where the foreign bodies that have been swallowed have produced fistulæ.

When a foreign body has got into the stomach you must consider it as out of your hands altogether, except that you must keep the bowels gently open. All violent purging

should be avoided; for if there be a sharp pin, great peristaltic action may cause it to do much injury. You may exhibit lenitive electuary or castor oil, but you must not be in a hurry to expel the substance, for it will generally pass after remaining in for a week or a fortnight, and if it be a small body it will come out much sooner. For the most part there is but little cause of apprehension, though in some cases unfortunate occurrences arise, as in the case of the woman who swallowed the pin. It is desirable to see that the substance does come away, and you must take care that the patient has his evacuations in a close-stool pan, and that they be minutely examined.

It has been proposed by the old writers to make an incision into the intestines, but at this time of day I do not think it is necessary to explain how much better it is to leave the case to nature than to have recourse to such a dangerous operation.

There is another matter of considerable practical importance to which I wish to call your attention, with respect to matters supposed to be stuck in the œsophagus. A woman was brought to town who was thought to have swallowed a piece of bone, and I believe that there was no doubt that she had done so. I introduced my finger, and, not being able to feel it, I concluded that it was below the reach of the finger. I then passed an œsophageal bougie into the stomach, but could not feel it; I then introduced a probang with a sponge, but with no better effect; but, still, the woman had the sensation of its being there. I now began to doubt whether it really stuck there, and to suspect that the sensation she experienced indicated that some part of the œsophagus had been abraded or torn by the foreign body, but that the body itself had passed into the stomach. It is a common trick with conjurers to put a half-crown into the hands of a person, to press it firmly, and then to say to him, "You are sure it is there?" The party says "Yes." In fact, he has the feeling of it, but when he opens his hand it is not there. The sensation made by the pressure on the hand remains a considerable time after the body itself has been removed, especially if the feeling be assisted by the imagination. You get a piece of sand or gravel into the eye; it is taken out directly, but you persist in saying that it is there; for a little inflammation of the eye produces a feeling as if a foreign body were on the conjunctiva. So I thought it might be with this patient, who imagined that she had a bone in the œsophagus which she could not swallow. Under that impression I ordered an opiate blister; and, under its influence, the sensation was, on the next day, very much abated; and, on the following day, was entirely removed. I think that the rapid subsidence of the symptoms under this treatment proved that they depended on an injury inflicted, and not on the foreign body remaining there. I met with a similar case in the following instance:—A maid-servant was supposed to have something sticking in the œsophagus, but, with the largest bougie or probang, nothing could be discovered there. I treated her in the same manner, and, in a day or two, the sensation was gone, and she was quite well. I suspect that this is not a very uncommon case. A person sends to you, and says that he has swallowed a fish-bone; you cannot find it; in reality, it has passed on; but it has pricked the œsophagus. By leaving such cases alone I have seen instances in which, in a day or two, the sensation has entirely disappeared.

REMARKS ON THE COMPARATIVE

VALUE OF AUSCULTATION

PRACTISED

WITH AND WITHOUT THE STETHOSCOPE.

By Henry Bennet, M.D., late House-Physician (*Interne*) to the Hospitals of St. Louis, &c., Paris.

Auscultation of the thoracic organs may be practised, as every one knows, either mediately or immediately. In the former case the stethoscope is used, in order to collect the sounds generated in the thorax and to transmit them to the ear of the auscultator; whereas, in the latter, the stethoscope is dispensed with, and the ear is immediately applied to the surface of the thorax. In France, at the present time, although auscultation is universally practised as an essential, indeed, indispensable means of diagnosis, the stethoscope is either entirely discarded in auscultation of the lungs, by the most eminent men of the day, and, consequently, by the great mass of practitioners, or otherwise is only employed in a very limited manner. In England, on the contrary, wherever auscultation is resorted to, either in private or hospital practice, you nearly always find that the stethoscope is used. The question which naturally presents itself to the mind of one who sees these two methods of examining the same physical phenomena resorted to in different countries is, whether the English or the French pathologists adopt the mode of auscultating most calculated to give a clear and correct perception of the sounds produced? It is this question which I shall endeavour to solve in the following remarks.

Before, however, I enter more fully into the subject, I must state, that although I perfectly agree with most of the valuable remarks on auscultation which appeared in a late number of The Lancet, as an experienced auscultator I cannot possibly admit "that every sound yielded by auscultation

is, to a well-trained ear, as distinct, though not so loud, as the stroke of St. Paul's bell." Would that it were so! for then the discovery of auscultation would be even a greater boon to suffering humanity than it has proved to be. Unfortunately, such is not the case, and if the author of the essay above alluded to had reflected more on the subject, I am sure he would not have written the sentence I have transcribed. Auscultation is an invaluable adjunct to percussion, and to the other means of diagnosis which we possess, at all periods of the existence of disease, from its commencement to its termination. But the clearness and distinctness of the symptoms which auscultation analyses are far from being the same in all the periods of thoracic affections. When the disease, whatever it may be, has once fairly developed itself, the symptoms are *generally* most clear, and most distinct, to any one acquainted with this means of exploration. In confirmed bronchitis, for instance, the sibilous or mucous râle; in confirmed pneumonia the crepitant râle; in advanced phthisis the dry crepitation, the mucous, cavernous sounds, &c., are all distinctly audible. But such is certainly not the case in the incipient stages of pulmonary disease, and in the regions of the lungs which are freshly attacked, or to which the disease progresses. Sometimes, indeed, I have known the sounds furnished by auscultation doubtful, during the whole course of a malady the nature of which was plainly indicated by other symptoms, as, for instance, in central pneumonia.

In the early stage of pulmonary disease, I should say that the symptoms furnished by auscultation are generally very difficult to seize, and are often very indistinct, even to a practised ear; so much so, that at a first examination we may be quite unable to appreciate their nature and meaning, however accomplished in the art of auscultating. A few isolated crude tubercles, of a certain size, or a number of minute miliary tubercles, deposited in a limited portion of the superior pulmonary lobe, may modify the respiration in such a manner as to enable a good auscultator to recognise their presence; but, in either case, the symptoms on which the diagnosis would be founded would be indistinct, and appreciated with difficulty. In the first case, if the tubercles were near enough to each other during inspiration and expiration, they might rub one against the other and produce a slight dry crepitation, only audible after a long expiration, or a fit of coughing. Now, this dry crepitation, although not heard during ordinary respiration, if found at several different examinations underneath the clavicle, or posteriorly in the supra-spinata region, would indicate pulmonary tuberculisation, even in the absence of dulness or of nearly any other symptom of that disease. I have, not once but many times, recognised the presence of crude tubercles in the lungs from this symptom alone. And yet the sound is often so indistinct, so fugitive, that none but a very practised auscultator would perceive it. In the second case, that of miliary tuberculisation, if the miliary tubercles are still fluid, the respiration will be weaker; if they have become solidified it will be stronger, harsher, I do not say than natural, but than on the other side. The intensity of the respiration varies so much in different individuals that we can justly say that there is no other standard by which we can judge such slight modifications than the state of other similar regions of the organ in the individuals themselves. I will give one other instance of the difficulty of correctly appreciating the sounds furnished by auscultation in incipient disease, derived from my own experience:—During a lengthened residence at La Salpetriére, which is both an hospital for the insane and an asylum for the aged, I had ample opportunities of studying, in all its stages, the disease which, of all others, is the most fatal to the aged, viz., pneumonia. The infirm and aged population of this establishment consists of more than three thousand old women. Among them are many who have for years been labouring under chronic bronchitis and emphysema. When the cold weather sets in, these affections generally become exacerbated, and those thus affected are often obliged to remain for weeks in the infirmary. It is amongst these old people that pneumonia principally appears, during the exacerbations I allude to, their residence in the infirmary not always preserving them from it. A strict daily scrutiny of the cases under my care enabled me often to observe the disease at its very onset, indeed, at a period of its invasion when it is rarely seen under ordinary circumstances. It was only by the presence of very indistinct, delicate phenomena, furnished by auscultation, that I was able to diagnose the impending invasion of pneumonia. In inflammation of all organs and all tissues, the first phenomenon observed is a flow of blood to the part, or congestion, which is usually termed inflammatory congestion. This taking place in inflammation of the lungs, as well as in all other inflammations, the result of the congestion is diminished calibre of the small bronchi and air-cells, and diminished conductibility of the lungs to the sounds generated within them. Fluids are bad conductors of sound, as every one knows; consequently, the greater the quantity of fluid contained in the vessels or cellular tissue of the lungs, be it blood, serosity, or pus, the weaker will the respiration be. Weakness of the respiration in a limited region of the lung, existing in a patient who presents acute febrile symptoms, especially if the patient be aged, indicates, therefore, to the experienced auscultator, the presence of incipient pneumonia; even when percus-

sion says little or nothing, and there is no other symptom which would in any way show the existence of visceral inflammation. And yet, in this instance, the morbid modification in the respiration is so slight as to necessitate a very careful comparative examination of the different regions of the lungs, and to be only noticed by one who has had great experience in auscultation. All who are acquainted with the rapid progress which pneumonia makes in the aged, and its intractable nature, will at once see how important it is to recognise the nature of the disease at this, its incipient stage. When actively treated, thus early, it may nearly always be subdued; whereas, when the lung is hepatised before the disease is discovered, a very large proportion of the cases treated must inevitably succumb.

These remarks apply with equal force and truth to cardiac disease. If you take a pupil to a patient labouring under an advanced stage of cardiac disease, presenting, for instance, hypertrophy and a tremendous rasp-sound, he will find no difficulty in recognising the morbid sounds, nor, indeed, would even a non-professional auscultator. But these are not the cases in which auscultation is most valuable. In confirmed disease of the heart, as in confirmed thoracic disease, auscultation might generally be dispensed with altogether, at least as far as mere diagnosis goes. It is in the incipient stages, when treatment is still available to suspend or destroy disease, that auscultation is really of inestimable value. It is the sounds furnished in these stages by auscultation of the heart which pupils should more especially study, for they are even more delicate and difficult to discriminate than the sounds heard in incipient pulmonary disease. I was, indeed, much surprised to find the late Dr. Hope stating that a pupil totally unacquainted with auscultation might become an expert auscultator of cardiac disease in a few hours. For the last four years I have been a teacher of auscultation in the Paris hospitals, and can say that although a great number of gentlemen have studied with me, most of whom were not mere pupils, but graduated members of the profession, I have always found that it took weeks, nay, months, to form *a really* expert auscultator, especially with regard to cardiac disease. Nor is this surprising when we consider that mere sounds, even when correctly heard, can only be properly interpreted by one who is thoroughly conversant with the *pathology* of the disease examined.

Now that I have, I think, fairly established by these examples, which might be much multiplied, that the phenomena furnished by auscultation at the commencement of thoracic affections are generally indistinct, fugitive, and of difficult perception, even to the best trained ear, we will revert to the principal object of this memoir; that is, to the examination of the comparative value of the different modes of auscultation usually practised.

Laennec's discoveries were made with the stethoscope, and up to the end of his career he almost exclusively used that instrument. Indeed, it was but natural that he should attach great value to the method which he had first resorted to in commencing his immortal researches, and that he should prefer it to all others. How great soever, therefore, Laennec's authority, on all points connected with auscultation, it cannot really be considered to have much weight with reference to the comparative value of a mode of exploration of which he was the inventor, with others which he seldom if ever employed. In order to form an unprejudiced opinion we must analyse thoroughly the various data of the question.

What end do we wish to attain when we resort to auscultation, either in the healthy or in the diseased subject? We listen to the sounds produced by the air in the bronchial tubes and air-cells of the lungs, and to those produced by, or accompanying, the passage of the blood through the heart. We endeavour to note their varied intensity, clearness, and intonation, to ascertain whether any abnormal sounds are produced, and what is their nature, affixing to each of the numerous modifications of which these phenomena are susceptible certain pathological meanings.

It is evident that the more intense are the sounds heard, the clearer will be the perception of them to the ear; and the intensity of sound depending, to a certain extent, on the the number of distinct vibrations heard, it follows that the more vibrations are heard the clearer will be the aural perception of them.

In the lungs the vibrations which occur in every region give rise to sounds which radiate externally through the parietes of the chest, and are so slight that they are lost in the surrounding atmosphere, without becoming sensible to the ear, unless they be modified by disease. If, however, the ear be immediately applied to the chest, or a tube of some hard substance, a good conductor of sound, be interposed, the sounds thus produced are distinctly heard. The distinctness with which the respiration is heard depending on the number of vibrations which arrive at the ear, it stands to reason that on applying the ear to the chest the respiration will be heard with greater distinctness than on using the stethoscope. The stethoscope merely covers a limited space of the parietes of the chest, and the sounds which are generated without of this area are not conveyed to the ear, or at least only their extreme undulations. When the ear is directly applied, not only does the concha of the ear render convergent and transmit to the ear the vibrations generated in the part of the lungs that it covers, but the temporal and

parietal bones which are in contact with the chest receive the vibrations of the regions they cover, and transmit them also to the auditory nerve. Thus, a much greater number of vibrations are heard than when the stethoscope is used, and the respiration is, consequently, heard more distinctly. That such is really the case may be very easily proved. Let any one listen attentively to the respiration of a healthy person with the stethoscope, and then apply his ear to the same region, using proper precautions in both instances, he will certainly find that the perception of sound is much more distinct, much more intense, when the latter mode of examination is employed than when the former.

It has been stated that mediate auscultation is much preferable to immediate when the lesion is limited, as in that case the stethoscope, by concentrating the attention on the precise spot where the morbid sound originates, enables the ear to analyse them with greater accuracy. Such, however, is not the case in auscultation of the lung. Not only are the morbid sounds themselves heard more distinctly with the naked ear, but their nature is more distinctly analysed when the sounds created in the healthy parts around are also conveyed to the auditory nerve. We have, then, a point of comparison, and by directing the attention alternately to the pathological sound and to the normal one, the differences which exist between them are more accurately perceived.

No unprejudiced person, who will give himself the trouble to compare the sensations produced on his own ear by mediate or immediate auscultation of the lungs, can, in my opinion, fail to give preference to the latter. There are, however, other and important advantages attached to immediate auscultation. However familiarised a physician may be with the stethoscope, it is an instrument the use of which requires some slight degree of care. Its inferior extremity must be placed in contact with the chest in such a manner as not to hurt the patient, and yet to apply firmly to the thoracic surface; the ear must, also, be properly fixed on the superior plate, and all this cannot be done without some little inconvenience and loss of time. Again, from the area taken in being smaller than when the head is immediately applied to the chest, a much smaller portion of the lungs is examined at a time; and if we wish to acquire a thorough knowledge of the state of the respiratory function in every region of the lungs, the stethoscope has to be so often applied that considerable time is consumed in the investigation. Owing, also, to the possibility of some error accruing from the wrong application of the stethoscope, if there is any doubt as to the sounds heard, we are apt to reapply it, and thus time is again lost.

When the ear is applied directly to the chest, the hair of the auscultator having been previously removed, the examination is performed with much greater ease. A considerable portion of the lungs being examined each time the ear is placed on the thorax, and much less time being necessary to analyse, mentally, the sounds produced, owing to their greater distinctness, the examination is made in a more satisfactory manner, and in much shorter time, than when the stethoscope is used. To the patient, also, immediate auscultation is a less formidable matter than examination with an instrument, however simple. In hospital practice the great rapidity with which a searching examination of the lungs may thus be made is invaluable. In a few seconds the state of the entire lungs may be ascertained with accuracy, whereas, were the stethoscope used, as many minutes would scarcely be sufficient.

Although I thus give a decided preference to immediate auscultation, it must not be thought that I wish entirely to discard the stethoscope in the examination of the respiratory organs. I firmly believe that the ear applied directly to the chest will perceive some slight modifications of the respiratory murmur, which it would not were the stethoscope resorted to; but beyond this I think that the stethoscope is adequate to all the purposes of diagnosis, subject, however, to the disadvantages above enumerated. Indeed, I fully concur with all authors who have written on the subject, that there are cases in which the stethoscope alone can be used, as, for instance, in the examination of the anterior thoracic regions in young females, of patients in a state of perspiration, or covered with vermin or filth. What I contend is, that no advantage is gained by the use of the stethoscope, with reference to the distinctness of the physical sounds examined; and that immediate auscultation, when possible, is infinitely more satisfactory and consumes infinitely less time.

I may also mention, as a fact which I have learnt from lengthened experience, that the less covering there is on the thorax of the patient who is examined, the better the respiration is heard, and the less is the auscultator liable to error. I am so fully convinced of the truth of this fact, that I nearly always apply the ear to the uncovered chest in men, when their state of cleanliness, &c. enables me to do so. With females the thinner the texture of the garment which covers the chest the better it is for the auscultator, especially when the symptoms examined are those of incipient disease.

The above remarks do not apply to the same extent to auscultation of the heart. If we merely wish to ascertain roughly the state of the heart, to know whether its sounds are normal or morbid, the ear applied directly to the cardiac region is the best and most expeditious means of investigation.

But if we wish to form an accurate diagnosis, to ascertain which of the valves or regions of the heart is the seat of disease, then the stethoscope becomes indispensable. This is easily explained. The cardiac region is not extensive, and the surface which is uncovered by the lungs is not more than one or, at most, two square inches in extent, in the healthy subject. Lying thus embedded in the lungs, it is only in this very small space that the sounds of the heart are absolutely free from the respiratory murmur. As we recede from it the respiratory sounds become more and more blended with those produced by the heart, until the latter are at last completely lost. If the ear be directly applied to the cardiac region, all these sounds are heard at the same time, as the ear and temporal region of the head not only cover the entire cardiac region, but also a considerable portion of the pulmonary organ. The respiratory murmur, not serving as a point of comparison, as is the case in auscultation of the lungs, interferes with the perception of the cardiac sounds. On the other hand, in the differential diagnosis of the diseases of the heart, it is of the greatest importance to ascertain whether it is the systole or the diastole which is modified; whether these modifications are heard in their greatest intensity at the apex or at the basis, on the right or the left side of the heart; and whether or not they are carried along the large vessels which pass off from that organ. The stethoscope only giving distinctly the sounds created underneath the area which it covers, enables us to exclude, to a certain extent, the respiratory murmur, in our examination of the sounds of the heart. It also enables us to examine separately, and to compare the varied nature and intensity of these sounds in each of the regions we have mentioned, which is scarcely possible if the ear cover at the same time both the base and the apex of the heart. Thus it will be seen that the same principles which induce me to discard the use of the stethoscope in auscultation of the lungs, whenever it is possible, also lead me to confine myself almost entirely to that instrument when examining the heart.

In auscultation of the sounds produced in the blood-vessels, arteries, and veins, the stethoscope must necessarily be resorted to. Their anatomical form and position renders it nearly always impossible to resort to immediate auscultation.

Having thus analysed the methods which are employed to appreciate the acoustic phenomena generated in the thoracic cavity, I shall conclude by a few words on the "study of auscultation."

Too much importance is attached by students, and often by their teachers, to the "use of the stethoscope." They seem to forget that auscultation merely furnishes us with the knowledge of *one* of the elements necessary to form a diagnosis of thoracic affections, and to think that the "stethoscope" is a kind of magical instrument which is to solve all difficulties. In reality, auscultation is not only useless to one who is not thoroughly acquainted with thoracic pathology, but may it be said to be a very Will-o'-the-wisp, only serving to perplex and to lead astray. The first thing to be done by a student is to acquire a comprehensive knowledge of the pathology of diseases of the lungs and heart. When he has done so he is able, and only then, to commence, with benefit to himself, the study of auscultation. Being thus prepared, it is of very little importance whether he use or do not use the stethoscope; provided he carefully listens to the sounds produced, analyses them by means of the pathological knowledge he already possesses, and diligently compares the symptoms thus obtained with the other local and general symptoms which the patient presents. I am, however, convinced that the first attempts to analyse the acoustic phenomena of the lungs will be attended with much greater success if immediate auscultation be resorted to, and that because the sounds are more distinct. It is wrong, also, to think that a student who has listened with the ear only would not be able to "use the stethoscope," as the phrase is. If his *mind* and *ear* have been once properly educated, he will be able to employ the stethoscope with as much ease and success as the physician who has never used any other mode of investigation. He knows what the sounds mean, and only wants to hear them to be able to interpret them. The stethoscope possesses no magical properties; it is merely a conductor of sound, and a good auscultator and pathologist who has never seen one in his life would hear as much on his first attempt as the man who had never auscultated without it.

If English practitioners could be convinced of these facts, auscultation would become much more general than it now is. Being divested of its apparent charlatanism, and rendered much less troublesome and tedious in its practice, the old as well as the young would universally adopt it as a means of diagnosis, as is the case in France. In French practice, at the present day, the examination of the thoracic organs by means of auscultation and percussion is nearly as much a matter of course as feeling the pulse; and by their assistance the physician feels a confidence in his knowledge of the state of these organs, which he certainly cannot experience if they are not resorted to. This state of things among our neighbours is principally to be attributed, in my opinion, to the stethoscope having lost its *prestige*, and to auscultation being viewed by them in its proper light.

There is another circumstance connected with the study of auscultation in this

country which, although apparently trivial, must exercise a baneful influence over its being extensively prosecuted by the students or hospital physicians; I allude to the extreme lowness of the beds in the hospitals. In all the hospitals which I have seen in London the beds are so extremely low, that anything like a careful examination of the entire thoracic region can only be made at the expense of great personal inconvenience, even when the stethoscope is used; and it is much worse when immediate auscultation is resorted to. I scarcely think that the increased facility which the patient experiences, in getting in and out of bed, compensates for the great disadvantages which it entails in a medical point of view.

Cambridge-square, Hyde-park,
Dec. 8, 1843.

ON THE

OBLIQUE MUSCLES OF THE EYE,

CONSIDERED AS

AGENTS IN ACCOMPLISHING

THE

FOCAL ADJUSTMENTS OF THE ORGAN OF VISION.

The power by which the eye is enabled to effect its varied focal adjustments has long afforded a theme of deep and attractive interest in physiological inquiry. With the investigations that have already been made the names of many illustrious characters are associated, and, consequently, it is with hesitation that one advances an opinion which will, in any degree, clash with theirs; still, however, whilst it is admitted by every candid observer that many difficulties yet remain unsolved, any attempt to throw further light upon the subject will not be regarded as impertinent or unwarrantable.

The opinions which have been held upon the means by which the optical adjustment to distances is accomplished may be classed under two distinct heads:—

1. Those opinions which consider the alterations in the focal adjustments to be effected by movements within the eyeball.

2. Those opinions which consider the muscles of the eyeball to be the agents in accomplishing the desired changes.

Before adverting to these views, however, it is necessary to state that an opinion is held by several physiologists,—and these, indeed, individuals whose names command the homage of the scientific world,—that no altered adjustment to distances is required, one adaptation alone responding alike to the object immediately beneath the eye, and to the object seated at the extreme verge of the visual range. No deep imbuement with scientific lore is, however, requisite to confute this opinion; indeed, it is sufficient to call to remembrance the existence of presbyopes or myopes, for by what means are we to explain their very existence, if the idea of focal adjustments in the organ of vision be abandoned?

In the hypotheses which have been proposed to explain the required adaptations by means internal to the eyeball, the difficulty is attempted to be surmounted in various methods; for instance, one view supposes that the desired changes are effected by a contractile power diffused over the entire sclerotica; another imagines that the active agent is a similar contractile power restricted to the cornea; a third, that the alterations are accomplished by a change in form of the crystalline lens, by an inherent contractility of its capsule; and a fourth considers the problem may be solved by supposing the position of the crystalline lens to become altered through the agency either of the iris or ciliary processes. In all these cases, however, since the most attentive examination has been unable to demonstrate the agents by which the supposed movements are effected, their existence must be regarded as somewhat hypothetical.

The opinions which consider the focal adjustments to be accomplished by alterations in the form of the globe of the eye, from muscular action, appear to be supported by more tangible arguments than the hypotheses we have just glanced at. The reason which particularly induces me to incline to this side of the question is the following:—The adjustment of the eye to distances is evidently an act of volition; now, as muscular tissue is the only structure in the animal organism which can execute movements at the behest of the will, the inference seems inevitable, that the careful investigation of the functions of the muscles of the eyeball will be most likely to be rewarded by a solution of our difficulties. Considerable contrariety of opinion, however, marks the writings of those physiologists who have advocated this hypothesis, so much so, indeed, that the most diametrically opposite actions are attributed to the same power; thus, by some it is imagined that the influence of the straight muscles will be seen in an elongation of the antero-posterior diameter of the eye, whilst by others their influence is thought to be of a directly reverse character; the same disagreement marks the opinions held on the action of the oblique muscles.

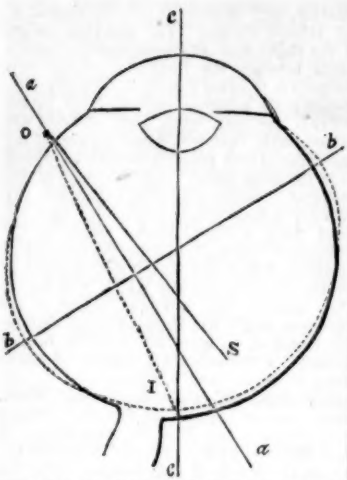
Having thus glanced at the various opinions which have been entertained upon the subject under examination, we will now proceed to the first step of our inquiry. This first step is most assuredly (although, strange to say, such has not been the invariable practice) to ascertain the condition of the eye when unacted upon by any cause of change; that is to say, we must ascertain the focal distance of the object to which the eye responds when it is in a *quiescent state*. I am not aware that this calculation has been

obtained with mathematical certitude, but the only conclusion which we can deduce is, that that is the unaltered or quiescent state which corresponds to the distance at which we see with the greatest distinctness and comfort. The position of this point is readily ascertained, and, although it is by no means uniform in all individuals, or in the same individual at all times, it is generally found to be about twelve or fourteen inches from the eye. That nature has made no provision for the contemplation of objects nearer than the point to which the eye responds in a quiescent state, appears clearly demonstrated by the following experiment: — If any object be placed in the natural focus of the eye we see it with the greatest degree of distinctness, but, as we bring it into closer contiguity with the eye, the outline becomes more and more indistinct, and this, too, in opposition to the most strenuous exertions of the will. If, then, it be found that the unaltered adjustment of the eye corresponds to the nearest object that can be contemplated with distinctness, it follows that all the focal adjustments which are required in vision must, of necessity, be of such a character as to require an approximation between the lens and the retina.

It being thus ascertained that all the focal alterations requisite for distinct vision must necessarily be of a diminishing character (*i. e.*, the antero-posterior diameter of the eyeball shortened), our next step will be to inquire concerning the means most likely to produce this result. In the remaining remarks it will be my object to show that this, perhaps, may be the function of the oblique muscles of the eye. In the investigation of this subject, however, we must divest ourselves of all prejudice and hypothesis, and proceed at once to the fountain-head—anatomy; as it is from this source alone that we can justly expect a happy issue to our labours.

In studying the physiological action of the oblique muscles, for sake of simplicity, they both may be described as passing (the superior oblique from its pulley, or point of action, and the inferior oblique about an inch below this, in the same vertical line, from its origin) from the inner wall of the orbit, at a point corresponding to the junction of the anterior with the middle third of the eyeball; and thence, the one superiorly and the other inferiorly, as passing backwards and crossing the eyeball in a line posterior to its vertical axis. The direction of the two muscles is not, however, strictly parallel to each other. The tendinous termination of the inferior oblique expands over that part of the sclerotica which is immediately external to the entrance of the optic nerve, or that part, in other words, which corresponds to the axis of vision. The superior oblique is also inserted on the posterior surface of the eyeball, but somewhat more external, and superior to the attachment of the inferior oblique. The probable design of the slight want of correspondence which exists between the insertions of the two obliques is to cause a more uniform approximation of the posterior surface of the retina towards the lens, when both obliques act commutually, than would have resulted if the insertions of both muscles had interwrapped over the same point. Another effect of this mode of insertion may be to counteract any rotatory action, which might else have ensued from a simultaneous action of the obliques.* Such, then, being the anatomical relations of these muscles, it now remains to inquire into the results which will follow their action. The action of either of the obliques, singly, has already been so ably investigated as to leave no room for further remark; a corresponding degree of attention, however, has not been paid to the simultaneous action of both muscles. What, then, may we inquire, are the effects which will follow a simultaneous contraction of both obliques upon a spherical body like the eyeball? No deep acquaintance with the laws of physical science is involved in the answer to this question. The change must necessarily be of the following character: *the diameter of the sphere will be lessened in the direction of the plane of muscular action, and the compensating increase of diameter must take place in a line or axis perpendicular to the plane of action.* The annexed diagram will perhaps render the subject clearer than any mere verbal description, however voluminous. Supposing, then, the diagram to represent a horizontal section of the eyeball, the *distinct black circle* will indicate the eye in a state of quiescence, and *the dotted circle* the same sphere when acted upon by the oblique muscles. The point O is intended to represent the upper extremity of a vertical line on the inner side of the orbit, on which are seated the pulley of the trochlearis, and, about an inch inferiorly, the origin of the inferior oblique. The lines O S and O I represent respectively the direction of the superior and inferior obliques, the line O I being dotted to denote the inferior passage of the inferior oblique with respect to the diagram. The line *a*, *a*, is the plane of the commutual action of the obliques, and the

* I am aware that *inversion* of the pupil has been thought by some to be the effect of the commutual action of the obliques. Such an idea, however, deserves but feeble support from the anatomical relations of the parts; indeed quite the reverse appears, and so far from any action of the obliques being to invert the pupil, it would seem evident that if any rotatory movement did result (which is very problematical) it must necessarily be of such a character as to cause *extraversion*, and not inversion of the pupil.



line *b*, *b*, the axis perpendicular to this plane. The line *c*, *c*, drawn through the centre of the eyeball, indicates the axis of vision. If now we consider the relations of *the dotted circle* of the diagram, which represents the globe of the eye, when acted upon by the oblique muscles, we find the diameter of the original sphere lessened in the direction of the line *a*, *a*, the plane of muscular action; and, also, as an inevitable consequence of the same action, *a similar shortening in the direction of the axis of vision*, indicated by the line *c*, *c*. The compensating increase is seen in the line *b*, *b*, the perpendicular of the plane of muscular action. The shortening in the antero-posterior diameter, or visual axis, which thus necessarily results, as a consequence of any contraction in the direction of the line *a*, *a*, appears to be the ultimate design of the movement, to the accomplishment of which, indeed, the contraction in the plane of muscular action is quite subservient. And, if we devote any degree of attention to the subject, we must admit that in order to effect a shortening in the antero-posterior diameter of the eyeball, the adjustment adopted by nature is the best, and, indeed, the only one, by which any power external to the eye could have been made subservient to such an end.

We have thus seen that all the focal adjustments in the organ of vision must necessarily be of such a character as to require an approximation between the retina and the lens, and also that the peculiar action of the oblique muscles *may be* to perform the focal adjustments. If this view, therefore, be not based on erroneous deductions, it is evident that the function of the oblique muscles of the eye is totally different from that of the straight muscles of the same organ. The office of the straight muscles would seem to be confined to the exclusive performance of the innumerable and essential movements of the globe of the eye, whilst that of the obliques is the accomplishment of the visual focal adjustments, which are equally, or even more, necessary to perfection and happiness, than any mere movements of the organ of vision; as we cannot fail to perceive that if no means had been ordained to the accomplishment of these focal changes, our power of distinct vision must necessarily have been limited to objects seated precisely at the distance of twelve or fourteen inches from the eye. Being thus debarred from the information which arises from a distant contemplation of nature, not only would the inlet of almost all our pleasure and knowledge have been cut off, but, maimed and incompetent, the faculty of vision would not even have sufficed to our preservation from the ordinary perils and difficulties which on every side beset our path.

A Student of a Provincial School.

Howden, Dec. 12, 1843.

CASE OF

ASPHYXIA, FROM HANGING,

TREATED BY BLEEDING.

VIEWS OF PRACTICE BASED ON PHYSIOLOGY.

By E. J. Shearman, M.D., Extra Licentiate of the Royal College of Physicians, &c., Rotherham.

On the 16th ult., at half-past one o'clock, p.m., a very powerful athletic man, named John Williamson, and his wife, were committed to Wakefield House of Correction, to take their trial at the next quarter sessions, for stealing a silver spoon from the house of the Rev. Wm. Speare, of Kimberworth. At two o'clock the same day, the man was found by the gaoler's wife, apparently dead, hanging by his own handkerchief to the grate of the prison, only forty inches from the ground. He was cut down. I saw him half an hour after the occurrence, and recovered him from the immediate effect of his suffocation; but he died at nine o'clock the following morning, without any sufficient evident cause. An inquest was held on the body on the following Saturday, and a verdict of "Temporary derangement" returned.

When I first saw him he was apparently lifeless; he neither breathed, nor moved, nor had any perceptible circulation. He was lying on the straw-bed in the prison, his face and neck much swollen and of a blackish colour, the ecchymosed mark of the cord being immediately below the thyroid cartilage; his fingers bent, and hands nearly clenched. I immediately had his head raised, and the windows and door opened, and opened a vein in his arm (*in preference to the jugular vein*,

because the room was very dark and inconvenient for that operation), and put his arm into hot water. A little blood flowed, which gradually increased in quantity as he became warmer. In four or five minutes he began to sob, and I let him bleed until *I could feel the pulse beat* at the wrist and the *pupil of the eye contracted completely* on the application of a lighted candle; *I then stopped the bleeding*. As he so soon began to breathe, there was no ammonia applied to his nostrils. His breathing remained stertorous. I poured some hot brandy and water into his stomach, applied warmth to his body and extremities, kept his head raised, and left my assistant with him at half-past three o'clock, to see that he had as much fresh air and warmth, as well as every other attention, that the nature of the situation would allow. At five o'clock I saw him again, and remained till half-past six. He had then rallied very much. His pulse had become firmer, but very quick, 130; his head was very hot; he was exceedingly restless and unmanageable, having very violent convulsive motions in both arms and legs, and he required more than four men to hold him on the bed; his breathing was stertorous, and he could not swallow. I put him on a straight-waistcoat, had his head shaved, showered it with cold water, applied an evaporating lotion afterwards, and gave him some croton oil on his tongue, which he kept within his teeth. My assistant remained with him until ten o'clock. I saw him again at eleven o'clock, and remained until half-past twelve. His head was then *excessively hot*; eyes *injected with blood*; he looked wildly round the room, and was so excited and strong that the straight-waistcoat did not keep him in order. The croton oil had operated on his bowels once; his pulse was very quick, small, weak, and jerking, or I should have bled him again in the arm or jugular vein. Twenty leeches were applied to his head; I continued the cold affusion and cold lotion afterwards, and kept him as warm as possible everywhere else. I left him, fully expecting there was so much congestion in the brain that it would end in phrenitis or meningitis by morning. Early the following morning my assistant saw him; he had then spoken several times, and was calmer; but very suddenly he became exhausted, and died at nine o'clock.

In this case I consider there was both *suffocation* and *apoplexy*. So much pressure was made by the cord round the neck, as to prevent inspiration, so that, *immediately*, the *lungs, right side of the heart, and veins would be congested*; part of the venous blood sent to the lungs through the pulmonary artery would be oxygenated during the first inspiration; but, when the next systole of the heart took place, if any blood passed into the left auricle by the pulmonary veins, it would be venous, *and if thrown out by the left ventricle*, would *act as a poison to the brain, and prevent the respiratory nerves from exciting the muscles of respiration*; thus, together with the pressure on the nerves and arteries in the neck, *producing asphyxia*. At the same time the blood was prevented returning from the brain in consequence of the ligature round the veins of the neck, thus *producing apoplexy*.

I, therefore, removed all pressure from the neck, encouraged the return of blood by raising the head and chest, removed the immediate congestion from the lungs and brain by drawing blood from a vein, and kept that flowing until the muscles of respiration were excited sufficiently into action to allow the air to change the venous into arterial blood, *as evinced by the activity of the brain*. When respiration had been established, it was my next duty to relieve the congested state of the brain, which I endeavoured to do by keeping the raised head (which was very hot) as cool as I could, and the rest of the body warm, relieving the congested state of the veins by local bleeding, and stimulating the mucous membrane of the bowels by croton oil. Still, the violent convulsive motions continued, which, I think, rather tends to point out the immediate cause of death to have been effusion on the base of the brain.

I immediately applied to the coroner for permission to examine the body, stating that it would be a most interesting inspection, and, on the inquest, I most distinctly said I could not account for the immediate cause of death *at the time it took place*; but, I am sorry to say, he would not allow me the privilege. On the inquest he said that he had been informed by an *eminent practitioner* in the neighbourhood that the man *ought to have been bled from the temporal artery instead of from a vein*; and there is a current report circulated that the man *ought not to have been bled at all*, in fact, *that he was bled to death!*

To satisfy myself as to the propriety of my mode of treatment, I took the earliest opportunity of reading the case before the Sheffield Medical Society, and put the following questions to them:—1. *Whether it would have been proper to open the temporal artery?* 2. *Whether there appeared to be any reason to suppose that too much blood had been abstracted?* 3. *If any better mode of treatment could have been adopted?* To all these questions I received a satisfactory reply. But, as this is a case the treatment of which does not admit of much time for reflection when it occurs, I may be excused endeavouring to prove that the mode which was adopted in the present instance was according to the most approved authorities.

It is generally acknowledged that the quantity of blood circulating within the cranium is nearly the same under all circumstances, and that the pathological condition as to plethora consists, principally, in want

of balance between the arterial and venous, *the arterial blood giving the natural stimulus to the nerves of the brain, when sent in its due quantity,—the venous, in excess, depriving the brain of that stimulus.* My old and respected preceptor, the late Sir Charles Bell, says,*—"The head and neck may be surcharged and bloated with blood, while the brain will preserve its natural condition as to the quantity of blood in its vessels." "*All qualities of life are maintained through the active circulation of arterial blood in the substance of the organ.*" "The veins of the neck being pressed upon, as by violently turning the head, by inverted position, or partial strangulation, apoplexy is induced. Here it is not an accumulation of blood in the brain, but an interruption and stagnation, which, equally with the rupture of a vessel, *influences the brain, and diminishes the nervous energies.* On examination of the brain in such cases, *the veins of the brain are turgid to an unnatural degree,* accompanied with a *corresponding compression of the arterial system. The blood is accumulated in the veins and the circulation arrested, and the activity of the arteries diminished proportionally.* We see, therefore, *how sense and motion are diminished and, at length, life extinguished, without rupture of a vessel. If this condition of the circulation could be, with precision, ascertained, bleeding from a vein would be preferable to opening the temporal artery.*" In this case *this particular condition* certainly *existed,* and I therefore bled from a vein.

In the celebrated work of Abercrombie on the brain,† page 202, *et seq.*, he gives several instances of what he calls simple apoplexy from strangulation, which were recovered by bleeding from a vein; and also many cases of apoplexy with convulsions, which were apparently recovered by the same means but shortly after fatal, without leaving any trace of disease behind. At page 286 he says,—"*In the treatment of apoplexy our remedies are few and simple.* Those in which our chief reliance is to be placed are, *large and repeated blood-lettings, active purgatives,* and *cold applications to the head,* aided by an *elevated position of the body, cool air,* and the *absence of all stimuli.*" "*The first bleeding should be from the arm.*" "*There are no symptoms which characterise a distinct class of apoplectic affections, requiring any important distinction in the treatment,* or, in other words, *a class which, in their nature, do not admit of blood-letting.*"

Paris and Fonblanque, Drs. Ryan and Beck, in their works on Medical Jurisprudence, and Dr. Roget, in the "Cyclopædia of Medicine," all recommend bleeding from a vein in cases of hanging.

There is a difference of opinion as to whether the jugular vein or one in the arm ought to be opened in asphyxia from hanging, which I will not enter into here. Had this man been placed in a favourable situation for opening the jugular vein, I should have done so at first; but I doubt whether he would have bled so well from it, as it was not until his arm was put into almost boiling water, and the muscles brought into action, that he began to bleed at all.

As to the charge of the quantity of blood taken being too large, I shall merely state that *four hours after he was bled, he had so much muscular strength left that six men could not hold him on the bed,* and if there is anything in the treatment of the case I at all regret, it is that, when those violent convulsions came on, I did not bleed him from a vein again, in spite of the peculiarity of his pulse.

The common treatment for asphyxia, viz., exciting artificial respiration, &c., would not, in this case, have been successful; the right side of the heart and lungs must have been gorged with venous blood, as well as the brain; and before that congestion was removed from the brain, the respiratory nerves could not act.

I trust I have satisfactorily shown that in asphyxia from hanging, bleeding from the temporal artery would *kill,* by depriving the brain of its proper stimulus. Bleeding from a vein, by taking off the pressure from the cerebral arteries and relieving the congestion in the lungs, would, on the contrary, stimulate the brain, excite the respiratory nerves, and restore animation; and no other mode of treatment would be likely to succeed until this has been accomplished.

Rotherham, Dec. 12, 1843.

PROPOSITIONS FOR

NEW METHODS

OF

ADMINSTERING ENEMAS AND OTHER INJECTIONS.

By James Carson, M.D., Liverpool.

Constipation of the bowels is the source of a large proportion of the ills that flesh is heir to, and not the least of the bad consequences arising from it is the employment of the remedial agents which are used to remove it. If accumulations in the lower bowel be prevented, all the intermediate machinery between the mouth and the anus, and the other excretory passages, by which the food is first formed into chyle and then into blood—in fact, by which the food is converted into man, and man resolved into the

* Practical Essay on the Questionable Practice of Bleeding in all Apoplectic Affections, and the Different Effects of Drawing Blood from the Artery and from the Vein, 1841.

† Pathological and Practical Researches on Diseases of the Brain and Spinal Cord. By John Abercrombie, M.D. 3rd Edit.

excrement—are easily performed. Therefore, the great desideratum for man is, in order to keep himself in good condition, first, to get and hold the nutriment of the food, and last, to get rid of the excrement. I do not know whether it be a fact, but it is said that the Egyptians deified the ibis on account of the important benefits it conferred upon mankind by teaching them how to administer a lavement. That bird, it is said, after gorging itself, is subject to indigestion, which it relieves by inserting its beak into the cloaca and injecting through it, or rather the nostrils, a stream of water into the lower bowel, thus relieving itself of the accumulated consequences of its gluttony. This mode of opening the bowels, viz., by enema, has been practised by all nations, but, perhaps, not to the necessary extent in this country, where the people have an extraordinary *penchant* for purgative remedies. Morison's, Parr's, *et hoc genus omne*. Patents are constantly being taken out for the requisite machinery for clysters, to which, however, there are very great objections. The process is a dirty one and not easily managed by the patient alone. Besides the difficulty of inserting the pipe or tube, great irritation in the termination of the rectum and the surrounding parts is produced by its presence, and the consequent, but abortive, efforts to defæcate prevent the introduction of a sufficient quantity of water into the gut.

The great desideratum is a tube which will produce the minimum of irritation and which may be easily introduced. Having turned my attention to this subject, my first plan was to cleanse out the bowel in the following manner:—Get a pair of water-tight breeches, made of strong Macintosh cloth, fitting tightly around the loins above, and the top of the thighs below, so that no water can, under the requisite degree of pressure, escape. The part of it that is immediately opposite the bottom of the anus opens into a flexible water-tight tube connected with an ordinary injecting apparatus. The warm water, which, in sufficient quantity, is by far the best enema, forced into the cavity of the breeches, must go somewhere, and if we can prevent it from escaping between the skin and the belts around the loins and thighs, I do not know where it can go excepting up the anus. We can make use of any degree of hydrostatic pressure, and, therefore, as a necessary consequence, we can overcome the resistance of the sphincter ani, and in the gentlest manner possible. Of course, in the female, it will pass up the vagina. This will often be a great desideratum, to enable us to wash out that passage and the womb, but, when not necessary, it can easily be prevented by placing a waterproof apron, something like a fig-leaf, over the aperture. I am not very certain whether, under these circumstances, the aperture of the anus being sealed by a similar contrivance in the male, the fluid, under sufficient pressure, would not pass into the urethra, overcome the resistance of any stricture, and wash out the bladder. I am certain that it would get into the urethra of the female, and it would only be necessary to insert a tube, of about an inch in length, into the orifice of the urethra, in the male, to attain the same object in him also. There are certain positions in which all these objects are effected with the greatest possible advantage: such an one is the posture of cowering down; and if a cast of the body in this position, be taken, a metal or earthenware vessel might be made, into the bottom of which a tube, connected with the injecting apparatus, might pass.

I considered it to be a not unimportant secondary use of the fleshy protuberances of the lower part of the body, that they would, sitting in such a vessel, prevent the passage of the water that is injected into such a vessel through any other aperture than the anus. A water-tight tunic, covering the whole body, would cleanse the pores of the skin and, in fact, every passage opening upon the surface of the skin, thus constituting a cheap and invaluable warm bath. Neither should I expect to see any strangulated hernia resist the gentle taxis of such a machine, by which the tumour is equally pressed outside and the intestine gently dragged out of its incarceration from within; nor, in the bladder, any stone resist the gradual wasting of a constant current of warm water passed, by such an instrument, continually over it surface.

But, on after consideration and trial, I found that the above machinery, unless subjected to some kind of improvement, would be quite inefficient for the purpose I wished. The breeches are inapplicable. It would be impossible to prevent the escape of water, under any degree of pressure, between the bands surrounding the loins and thighs, by any simple bandage. If you want to exert an uniform pressure upon any surface, you must take a disc accurately fitted to the surface. This disc may be formed of any substance, but the edges ought to be as inflexible as possible. Let the edges be a metallic or wooden rim. To the lower side of this rim attach a waterproofed cloth, made of Indian-rubber, between two other cloths, the upper being cotton, the lower washleather. To the upper side of the inflexible rim you attach a bag, of any size, made of waterproof cloth, of strength proportioned to the degree of pressure you wish to exert upon the surface. Let this communicate with a waterproof tube, of any length, but of proportionate strength to that of the bag. A column of mercury, a column of water, or a forcing-pump throwing either of these liquids into the bag, will exert any degree of pressure required uniformly over the whole surface. As the disc may be adapted

to any form and extent of surface, you may have a means of exercising, on any part and to any extent of the external surface of the body, an uniform pressure to any degree. If you wish to inject a fluid into any aperture of the body you have only to cut a hole in the disc opposite to the aperture, the edges of the hole to be affixed round the aperture by some slightly adhesive substance, as simple cerate. It would be well, under all circumstances, to grease the surface of the disc with the same substance.

What are the uses to which, in surgery, an uniformly-exerted pressure on any part of the body may be applied? In the first place, I think it is incontestible that the injection of a fluid into any one of the cavities of the animal body which open on to the surface may be effected without a tube. In the second place, an uniform pressure may be exerted upon the abdominal parietes, by which the evacuation of the contents of these cavities may be facilitated. And in the third place, an uniform pressure, exerted in all directions upon different parts of the body, will oppose the greatest possible obstacle, or the best remedy, to swelling or dislocation of any description. The objects gained by the first set of processes are obvious. By the second set of processes, the bowels, the bladder, the vagina and uterus, the stomach, and, if necessary, the lungs, mny be evacuated of their contents, whether they come there by natural processes or are thrown in by the first set of processes. Among the objects attained by the second set of processes, is the assistance afforded to the parturient female by the exertion of an uniform moderated pressure, assisting the pains and promoting the progress of the child in their intervals. I think this may be, with due reverence, assumed as a step to the removal of the original curse inflicted upon that interesting sex, "In sorrow thou shalt bring forth children." Among the advantages to be derived from the third set of processes are the prevention and reduction of all kinds of ectopia, aneurisms, hernia, swellings from malignant or non-malignant tumours, dislocations, fractures, swellings from inflammation, hæmorrhages from cut blood-vessels, &c. &c. I am prepared to answer any objection which may be offered to any one of these applications of an equal and uniform pressure upon the surfaces of the animal body. Without being much of a naturalist, I predict, with perfect confidence, that each and every one of these applications are made use of in some part of the animal kingdom to remedy accidents and to remove inconveniences. The most fruitful field for investigation will be found among the microscopic infusoria. He has very little imagination who does not see that any necessary degree of pressure, above or below that of the atmosphere, may be exerted by this apparatus. You have only to convert your pressing column of mercury or water into an exhausting column, or your forcing-pump into a suction-pump, and any requisite degree of pressure less than that of the atmosphere is produced. I believe that among the uses to which I have applied the modifications of pressure to the whole or part of the surfaces of the animal body, are comprehended the greater part, if not all, the assistance which may be afforded to man and other animals by physicians, surgeons, and apothecaries. The consequences are obvious.

A word upon the history of this subject, as far as I know anything about it. About thirty years ago, my father, Dr. Carson, invented a machine, like a slipper-bath, for diminishing or increasing the atmospheric pressure from the surface of the body. The apparatus was constructed for him by the respectable firm of Leishman and Welsh, of this town, and when, ten years afterwards, I was apprenticed to the surgeon of the poor-house in this town, its dilapidated remains still existed. The first patient who was placed into it was nearly strangulated by the collar fitted round the neck, and my father had not the practical knowledge of mechanics sufficient to remedy the defect. He afterwards sought to effect the same purpose by sending some of his patients down in diving-bells, one of whom I accompanied; but I conceive the case to be one in which, for particular reasons since ascertained, a dense atmosphere would be rather prejudicial than otherwise. At any rate, it did not do him any good. At the meeting of the British Association in Liverpool, Sir James Murray laid before the medical section a series of apparatus for exercising different degrees of pressure on the several parts of the body. Among these, without any possible knowledge on his part of what had been done in the same way by my father, was his slipper-bath, but with a non-strangulating collar. Many of the objects which I have proposed in my method of exerting pressure, were suggested by Sir James as attainable by his apparatus. I have not the slightest doubt that that very ingenious physician fully comprehended all the advantages attainable by these processes. The only objection to his apparatus was that his machinery was expensive and cumbersome. Perhaps my own processes have, in part or altogether, been suggested before; but if so, they appear not to have been appreciated. If they have, all I can say is that this is an old song to a new tune, which may, perhaps, make it more acceptable.*

Liverpool, Dec. 13, 1843.

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* Dr. Arnott suggested and, I believe, put in practice, a method of overcoming stricture in the urethra, by the pressure of water. His instrument I have not seen.

CASES OF

PULMONARY CONSUMPTION,

IN WHICH

NAPHTHA WAS EMPLOYED WITHOUT SUCCESS.

By GEORGE P. MAY, M.D., Maldon.

THE record of facts is the sure method of testing the value of theories. The therapeutic powers of remedies, as well as their physiological action, can be estimated only by extensive observations; and it becomes the duty of every practical man to contribute, so far as his opportunities will allow, to the accomplishment of this end. When, under the imposing epithet of "specific," with the impress of respectable authority affixed to it, a remedy is vouchsafed to the world, in an especial manner does it behove every practitioner, as the natural guardian of the public health, to examine its claim to such a title; and, by careful investigation, conducted without prejudice, to satisfy himself of its value, before its adoption by a credulous public, whose grand characteristic is exhibited in an almost unlimited confidence in any remedy which wears the garb of novelty, provided it be lavish of promises, without exception or reservation.

Of all the diseases in the long catalogue of ills that flesh is heir to, it is well known that there is none which has so baffled the skill of the professor of the healing art, and set at nought his appliances, as *pulmonary consumption;* and, consequently, few have offered a finer field to the charlatan and quack for the exercise of their vocation. True it is that the investigations of pathologists of the current century have furnished us with an accurate view of the nature, origin, and progress of this dreadful malady, but still as much as ever it sets the resources of our art at defiance, still it continues to be the "opprobrium medicinæ," and with all the light of pathological truth beaming around us, we can no better than our ancestors prevent the deposit of the insidious granule, or, with any degree of certainty, check the progress of its destructive vegetation. Thousands of remedies have been proposed, and have, in their turn, been lauded, decried, and forgotten. Amongst modern claimants to the discovery of a specific for tubercular phthisis, Dr. Hastings holds a prominent place; and, indeed, if the extraordinary success which has attended his mode of treatment, as practised by himself, could be realised in the hands of others, his temples would be entitled to a wreath more durable than ever decked the victor's brow, and his memory to the benison of generations yet unborn. Previously to the appearance of Dr. Hastings' work, I commenced the naphtha treatment, and, after an attentive perusal of the book (although some of the cases there detailed appeared to me to be deficient in many of the physical signs of genuine phthisis), I was not without some hopes that at last we had placed in our hands an agent which, if not in all cases capable of effecting a cure, at least would have a decided influence in ameliorating some of the distressing symptoms attendant upon the disease. These hopes, unfortunately, have not been realised. I subjoin a very brief abstract of each case in which I gave it, and I would premise that the naphtha was procured from Bell and Co., in Oxford-street, the source recommended by Dr. Hastings.

CASE I.

July 14, 1843. A. N., ætat. 32, has had cough and expectoration of opake yellow matter for some months, with emaciation. Was able to pursue her occupation, that of a dressmaker, until the spring of the present year, when her debility was much increased. Percussion dull over the left side throughout its whole extent; respiration cavernous in the same side as low as the third rib; pectoriloquy perfect; percussion natural on the right side; respiration bronchial; no râle. To take ten drops of naphtha three times in a day. After taking it a few days, she "thought it did her cough good." She continued the naphtha without the least apparent benefit until within a few days of her death, which took place Sept. 11. Whilst taking the naphtha, the night perspirations were much increased.

CASE II.

July 13, 1843. S. M., ætat. 20; four months ago, being in service in London, was attacked with cough, accompanied with much debility and languor; returned home a fortnight since. Countenance unhealthy; expectorates globular masses, of a greenish-white colour; pulse rapid; perspires much at night. The subclavicular spaces on either side are much flattened, and preternaturally sonorous on percussion; under the right clavicle the inspiration is accompanied with a single moist crepitation; pectoriloquy perfect. On the left side the respiration is rough and grating, and a puff is heard to strike at the bottom of the cylinder at each effort at articulation; sounds of the heart diffuse. After taking ten drops of naphtha three times daily for three weeks, the expectoration somewhat lessened, but again increased in a few days; the dose was augmented to fifteen drops. Colliquative diarrhœa set in, which was with difficulty checked by opium. As she considered the naphtha aggravated the diarrhœa, she was ordered to omit it for a few days, but as little alteration was observed in the frequency of the motions, she resumed it; becoming, however, rapidly worse, she finally abandoned it, after having taken it about a month. Died Dec. 3.

Case III.

Aug. 18, 1843. Mrs. S., ætat. 30, had been affected during the last winter with cough and shortness of breath, which confined her to her house. During the summer her health improved, so that she was able to take walking exercise in the open air. Towards autumn her health again declined; she was troubled with night perspirations, cough, and expectoration; catamenia suppressed. Percussion elicited a dull sound under the right clavicle, in which situation a small crepitation is heard at the end of each inspiration. On the left side the inspiration is broken. Took the naphtha, in doses of fifteen drops, for nearly a month, when it was abandoned at her own request. During this time the symptoms progressed from bad to worse without any mitigation. She is now entirely confined to her bed, and her death is daily expected.

Case IV.

July 20, 1843. O. J., ætat. 24; family consumptive; had dry cough at the beginning of the same year, which did not much interfere with his health until the middle of May, when he was attacked with hæmoptysis, which lasted more or less for two days, since which time he remained much debilitated. Face pallid and thin; coughs much, and expectorates mucus with yellowish masses suspended in it; is capable of walking about, and can lie on either side; pulse 94; perspires at night; percussion rather dull in the left subclavicular region, as far as the third rib; respiratory murmur in this situation feeble, and occasionally broken; a faint crepitation may sometimes be detected at the termination of each expiration; much vocal resonance; percussion under the line of the right clavicle slightly dull; respiration over the whole of this side puerile; no râle. He commenced taking naphtha, three times daily, in doses of ten drops, which were gradually increased to twenty. The only effects observed were that it produced severe griping, aggravated the diarrhœa, and, while under its use, perspirations were most profuse. To control its tendency to increase the diarrhœa, a few drops of laudanum were given in conjunction with the naphtha.

With much perseverance this patient persisted in its use, although to him a very disagreeable medicine, until the 1st of September, a period of six weeks, when he could not be persuaded to go on with it longer. During the time he was taking it, the cough, expectoration, hectic fever, and emaciation increased rapidly; and stethoscopic examination discovered progressive softening and disorganisation of the upper part of the left lung. He died in October.

Case V.

Aug. 18, 1843. Mrs. S., grocer, of stout, robust make, has for several winters been troubled with cough and expectoration. In the summer of the present year spat blood for two or three days, since which has had much expectoration of muco-purulent matter, frequently tinged with blood; has lost flesh, but is still capable of partially serving in her shop. Percussion dull under the left subclavicular space, which is considerably flattened, and in which situation a moist crepitous râle is heard. To take twenty drops of naphtha three times a day.

Sept. 28. Is evidently weaker; cough violent; expectoration copious, and much dyspnœa on exertion. States that the only effect which she can ascribe to the drops is the induction of some degree of headach after taking them.

After persevering in the use of naphtha nearly two months, without benefit, she discontinued it, at which time the crepitating râle had given place to a strong and extensive gargouillement. Her condition is now much the same as when she discontinued the use of naphtha.

Case VI.

Aug. 12, 1843. Mrs. S., ætat. 29, has had cough since last Christmas, but without much disturbance of the general health, until about three months ago; she then experienced debility and night perspirations. Her present symptoms are as above detailed; expectoration sparing; pulse very rapid; catamenia suppressed for two months; percussion very dull below the left clavicle, inspiration here terminating in a slight mucous râle; expiration prolonged, rough, and blowing; pectoriloquy distinct. After taking the naphtha in doses of twenty drops, for a month, without any benefit, and with rapid advancement of the disease, she discontinued it.

Case VII.

Oct. 1, 1843. J. F., ætat. 32, states that he has had cough for some weeks; expectorated recently about a teaspoonful of blood; suffers from shortness of breath, but is tolerably robust in appearance; on percussion a trace of dulness is perceived immediately under the right clavicle; the inspiration here is feeble; expiration broken. After he had taken twenty drops of naphtha three times a day, for a fortnight, with little or no relief to his cough or breathing, I did not feel justified, after the naphtha had hitherto entirely failed in my hands, in omitting any other means which I thought might be beneficial to the patient. I therefore kept up counter-irritation under each clavicle for several days; at the same time he continued the use of the naphtha. Under this plan his health improved, his cough moderated, and his breathing became less difficult, and for the last three weeks he has been able to fulfil the duties of his avocation, that of a farm-labourer. He ascribes his recovery to the

repeated blisters. Whether these, or the naphtha, be entitled to the credit, I shall forbear to express any opinion.

From an attentive consideration of the cases which have been imperfectly detailed above, I cannot hesitate to state that, at present, I am sceptical as to any beneficial influence exercised by naphtha in cases of true tubercular phthisis. At the same time I shall not fail to give it a more extended trial, as opportunity offers.

Maldon, Dec. 20, 1843.

CASE OF

RECOVERY FROM A COMPOUND FRACTURE OF THE SKULL.

To the Editor of THE LANCET.

SIR,—I beg the insertion of the following case of injury of the brain in your valuable Journal, if sufficiently interesting, and I have the honour to be, Sir, your very obedient servant,

A. BANON, M.D.,
Surgeon to Jervis-street Hospital.
Mountjoy-square South, Dublin,
Dec. 18, 1843.

John Cullen, ætat. 12, admitted into Jervis-street Hospital, October 1, 1843. In the morning of that day he was knocked down and stunned by a kick from a horse on the left side of the head. He was immediately carried by his friends to the hospital; he managed, with some assistance, to get up stairs, and staggered into the ward. On examination there was found a compound and comminuted fracture, engaging the upper and anterior part of the left parietal bone near its anterior and superior angle. It was a flap wound, patent and crescentic, with its convexity towards the temporal bone. The posterior canthus of the fracture was particularly indented, having probably been opposed to the cock of the shoe. The bone was here forced through the dura mater upon the brain. Some of the brain had escaped through the wound, and was scattered on the scalp. No other injury was detected. The boy was conscious, and apparently in possession of all his faculties but that of speech; general symptoms not striking; respiration tranquil; pulse regular, but slightly accelerated; has vomited since the accident; stomach and bowels now undisturbed. On consultation with the other surgeons of the hospital, it was decided to leave matters as they were, and not to attempt the elevation of the depressed bone. The symptoms were not urgent, and so circumstanced was the fracture that any attempt to raise or remove the bone would only be partially successful, and additional injury might be done to the already lacerated brain; besides, experience has proved that cases like this have done well without the interference of the surgeon. The head being shaved, and cold lotion applied, he was ordered bleeding to twelve ounces.

℞ *Powdered jalap*, eight grains;
Protochloride of mercury, three grains;
Powdered ginger, two grains. Mix for a powder, to be taken immediately. A purgative enema to be administered at night if the bowels be not previously moved; poultice to the wound; strict injunctions given that he should be carefully watched.

2. He slept badly last night; is drowsy to-day; pulse 60, and a little hard; face flushed; aphonia persists; tongue, on protrusion, appears to be slightly drawn to the right side. The usual signs of paralysed motor energy on the right side of the face are faintly traceable. I determined on following the plan of Mr. Carmichael, of this city, in similar cases, of bringing the system rapidly under the influence of mercury, and accordingly ordered—

℞ *Protochloride of mercury*, six grains;
Powdered refined sugar, twelve grains. Mix for six powders; one to be taken three times a day. A dozen leeches to be applied to the temples.

3. No remarkable change; complains of headach. Ordered six leeches to the temples, and to continue the powders. Pulse slightly hard, and 60; makes no attempt to speak; he is drowsy, and rather irritable when spoken to, but perfectly conscious. Vomiting recurred this day.

4. Still in the same state of listlessness; gums slightly sore; well-marked mercurial fetor; bowels disturbed with involuntary fæcal dejections; pulse 48, slow, small, and weak; aphonia persists; complains of pain in the head; discharge from wound copious; some granulations sprout on its circumference.

℞ *Castor oil*, half an ounce;
Tincture of rhubarb, half a drachm;
Tincture of henbane, eight drops;
Cinnamon water, an ounce. Mix for a draught, to be taken immediately. Omit the powders.

6. Still listless and feeble; pulse 40, and weak; slight strabismus of both eyes is discernible. Can now, for the first time, make use of certain monosyllables, as No and Yes.

8. Gums still sore and swollen; appears anxious for food; slept well last night; complains of headach; bowels regular; pulse 44. Ordered six leeches to the temples.

10. Improved in every respect since last report; countenance more lively; pulse 52, and soft; copious and healthy discharge from wound; tongue clean, and bowels regular. He is more alive and impressed by external objects; attempts to answer ques-

tions, and sometimes succeeds in stammering a word.

16. For the last three days he has evidently been losing ground; more lethargic than before; is quite conscious when spoken to; sleeps constantly; no stertor; pulse 40, and weak; discharge from the wound scanty,—towards evening it was observed trickling down his cheeks, and when the poultice was removed it was seen welling up from the wound in great quantity. This increase of discharge was attended with marked improvement in the symptoms; the pulse became more frequent, and the countenance more intelligent and lively. It appeared to me that the matter must have been pressing upon the brain and aggravating the symptoms since the 10th. His present improvement he probably owes to its having since fortunately made its way out through the wound.

18. Not so well as at last report; he is weak, irritable, and listless; discharge from wound scanty. Blister to the back of the head.

20. Discharge from wound abundant; his state is much improved; appears to be slowly regaining his power of speech; all traces of paralysis of the face and tongue have vanished; the wound is healing; pulse 66, natural.

Dec. 4. Our patient has continued to improve since the last report; so much so as to warrant his discharge from the hospital on this day. He gradually regained his powers of speech, though he finds a difficulty still in pronouncing some words. All his faculties seem to be now in full vigour; he is a remarkably intelligent boy. The wound has healed, except at one point, where apparently a small exfoliation of bone is about to take place.

18. He has walked several times to my house, a distance of about a mile. Appears to enjoy perfect health. A small exfoliation of bone has taken place, which will probably be followed by another.

CASE OF

FACIAL NEURALGIA

SUCCESSFULLY TREATED BY

ACONITINE.

To the Editor of THE LANCET.

SIR,—I should feel obliged by the insertion of the following case in the pages of THE LANCET, from the fact of its having resisted the whole class of remedies that are generally employed in neuralgic affections, and from its exhibiting, in a remarkable degree, the beneficial effect of aconitina in nervous diseases. Should further experience confirm our opinion respecting the use of that remedy, it will prove a valuable addition to the materia medica. I am, Sir, your obedient servant,

W. P. BROOKES, M.D., M.R.C.S.E.
Resident Surgeon to the Cheltenham General Hospital.

December, 1843.

June 1842. Miss ——, æt. 17, of nervous, hysterical temperament, has for some months been troubled with severe paroxysms of pain affecting the left side of the face. At the present time she complains of pain extending over the whole of the left side of the face, commencing just below the orbit, and reaching to the alæ of the nose and upper lip. The left side of the lip is much drawn up. The pain is not accompanied with the slightest permanent discoloration of the skin; but at the commencement of each paroxysm the skin of the affected side becomes white, whilst that on the other side of the face retains its natural colour. This blanching of the skin is accompanied with a sensation, as of cold water trickling down the cheek; the pulse at such periods is slightly intermittent. Even slight pressure with the finger gives her excessive pain, and there exists some tumefaction of the affected side. Sometimes she experiences several paroxysms during the day, but at other times an interval of a week or more may intervene between their occurrence. They are sure to make their appearance on her taking the slightest cold. When they occur in their greatest intensity she generally becomes hysterical, and rigidity of the neck and arms ensues. During these paroxysms she abstains from speaking or eating solid food, as both actions cause considerable pain. The functions of the digestive organs do not appear deranged, and the general health is as good as usual. The symptoms cannot be traced to any affection of the brain.

Belladonna, veratrine, strychnine, and iodine, were all tried without affording relief. A blister to the back of the neck gave some temporary ease, but after a few hours the symptoms returned with their original severity. Quinine and almost all the remedies usually employed in nervous affections, were internally exhibited without benefit. It was at last determined to use aconitine, and the following formula was employed:—

℞ *Aconitine*, two grains,
Rectified spirit of wine, as much as sufficient to mix it with two drachms of lard for an ointment. A piece, the size of a pea, to be rubbed into the face on the access of the pain. The application of the ointment caused what she termed a twitching, stinging sensation of the skin, and after a few trials the paroxysms became diminished, both in their frequency and severity. The aconitine was used six times or oftener, daily, during nearly three weeks, with marked benefit, and continued for some time afterwards, longer intervals being gradually

allowed to elapse between each application. The pain, from the period of commencing its use, progressively diminished, and has now nearly ceased, only appearing on her taking cold. On visiting her the other day, she informed me that she had not had an attack for more than three months; and that when the pain did come on, she had recourse to the ointment, which afforded immediate relief.

Remarks.—We generally find that neuralgia occurs in parts covered with dense fascia, and the disease has been supposed by some authors to be an inflamed state of such membrane, or of the periosteum covering the bone of the affected part. If such were the case, should we not expect benefit from the use of counter-irritants and local depletion? But these remedies frequently aggravate instead of diminishing the pain. To constitute true "tic douloureux" the pain must be confined to a certain part of the face—that to which the branches of the fifth nerve are distributed. The turgescence of the face in the case reported above, amounted sometimes very nearly to a species of inflammation, but was not marked by true inflammatory symptoms. When the acute pain ceased, the soreness of the skin rather increased; and the latter will frequently remain after the restoration of the patient to comparative health. The aconitine must be persevered in until all soreness disappears. The sensation of water trickling down the face, and the intermittence of the pulse, were the sure precursors of the paroxysms, and I am inclined to consider them as a cold stage of the disease. Though for the most part met with in the opulent, the disease, if watched for, is to be observed in the poorer classes, who generally term the disease "rheumatism in the face," and seldom apply for relief to medical men.

TRANSCENDENTAL ANATOMY.

STRICTURES OF PROFESSOR VALENTIN ON THE VIEWS OF DR. MARTIN BARRY.

To the Editor of THE LANCET.

SIR,—There appeared in THE LANCET for October 1, 1842 (page 35), under the head of "Discoveries in Embryology," a series of extracts from Professor Valentin's "Repertorium," tending to give support to the theories of Dr. Martin Barry. Your non-German readers, Sir, may suppose that Valentin's ideas on the subject are the same now as then, and thereby do great wrong to that eminent man, whose more mature and well-considered opinions are somewhat pithily expressed in the following extracts from Part I. of the "Repertorium" for 1843, to which, I trust, in justice to the professor, you will give insertion. I remain, Sir, your obedient servant,

ANTI-SPIRAL.

December, 1843.

"Barry imagines he has discovered a new, universal, fundamental law of structure. According to him, there exist in animals primary and secondary fibres and filaments, which twist about with a spiral course. According to this view, parts (*e. g.*, the blood corpuscles), in which nobody ever saw a fibre, do, in reality, contain them. Well known as it may be, however, that many fibres of human and animal structures are disposed screw-wise, yet the views of this author bear far too much the stamp of a lively fancy, and plainly show that his eyes, assisted by very high powers, believed they saw many things, which, in accordance with preconceived theories, they ought to have seen. To favour these views many notions, long since become matters of history, have been anew raised up."—P. 6.

"The work of the first (Bischoff) which has been already published, treats particularly of the ovum of the rabbit, including, however, that of other animals, and contains a valuable collection of researches on the delicate subject of the first development of the mammiferæ. More especially does it tend to give to the researches of Barry, of which we have treated in the last number of the "Repertorium," the appearance rather of offsprings of a fertile fancy than that of results of cool observation. Involuntarily does the idea force itself upon us, that the English investigator, led away by many theoretical views,—for example, upon the division of the nucleus, and new cell-formation round it, upon the process of endogenous cell-formation, and especially by new cell-theories,—believed that he really saw what, according to his views, ought to exist."—P. 17.

CEREBRAL MISGROWTHS.

To the Editor of THE LANCET.

SIR,—Some weeks ago (page 184) a communication from the pen of Mr. Barker, of Northwich, was inserted in THE LANCET relative to the case of a child said to have been born with a double brain. At the time of reading the letter I was forcibly reminded of a case which occurred in my own practice about twelve months ago. Mine was a fine instance of *hernia cerebri*, and I have little doubt, from the situation of the tumour, and from the manner in which the cerebral mass protruded from the cranial cavity in Mr. Barker's case, that that also was an instance of the same disease. Perhaps Mr. Barker will have the kindness to inform me, and your readers generally, if the protruded portion of brain contained separate

and distinct ventricles; whether there were the several pairs of nerves issuing from its base, as, also, from the base of the portion of the brain in the cavity of the cranium; and if each portion possessed a separate pons varolii and medulla oblongata. If these parts were alike, and distinct, in both portions of the brain (or, rather, in each brain, as we must then, undoubtedly, consider them), it would be interesting to know precisely the relation borne to the natural brain by the supernumerary one; whether the anterior part of the supernumerary brain were in contact with the posterior part of the natural brain, or whether the posterior surfaces of the two were in approximation. If the several pairs of nerves were distinct, whither did they proceed, and where terminate. If each brain possessed a pons varolii and medulla spinalis, at what point did the additional spinal column become blended with that of the normal brain? After dissection were the parts preserved?

I am induced to put these questions to your correspondent because nothing was stated in the original letter to lead me to infer that it was a case of *double brain*, and in consequence of its similarity, so far as I can infer, to the case before mentioned in my own practice. In former times there is no doubt that a great number of *lusi naturæ* were published as very marvellous and monstrous productions, which would have been proved, by the more minute and accurate anatomical investigations of the present day, to have been very simple, and easily accounted, for aberrations from nature.* The case of hernia cerebri to which reference has been made, I may transcribe at my leisure from my case-book, and submit to the notice of your readers, with a sketch of the tumour. I sent this to an eminent physiologist in London, who made an accurate examination of the parts, and deposited the preparation in one of the museums. Trusting that your correspondent will receive these observations in the same spirit as that in which they are written, I am, Sir, your obedient servant,

T. H. B.

Dec. 29, 1843.

PROLONGED CONTINENCE ALLEGED TO BE A CAUSE OF SPERMATORRHŒA.

To the Editor of THE LANCET.

SIR,—Now that Drs. Ranking and Dangerfield have, in THE LANCET, anticipated the call of the profession for more general information on points heretofore abandoned to empirics, we have Dr. Bull, "with fine frenzy rolling," reverting his eyes to heaven, with uplifted hands, the veriest similitude of a fanatic, manifesting his horror of an outrage committed against morality by Dr. D., in his curative recommendation of sexual intercourse, for inveterate onanism and spermatorrhœa resulting from prolonged continence.

Although matrimony was not stated by Dr. D. in his essay, it is *unfair* to *distort* the remedy into an *illicit* one. "If sexual intercourse," asks Dr. Bull, magnanimous in wrath, "be absolutely necessary for the preservation of health, what is to become of religion and morality?" The reply is obvious; in the apostle's advice, "Rather than burn, marry." Should Dr. B. refer to the volume of the sacred law, he will find both Moses and St. Paul more practical physiologists than himself in a moral point of view. The wrath of heaven visited the offending Jews for their abandoned profligacy, and matrimony was gravely recommended to the incontinent. Drs. Lallemand and Dangerfield have merely done the same; therefore, what was virtue in the two greatest moralists the world has produced, can scarcely be sin in the physicians alluded to. The morale of this subject, however, is such sickly cant that it requires no serious refutation; I will, therefore, hasten to the chief questions at issue.

The first, "continence *never* a cause of spermatorrhœa."

Many an adult male, and, perhaps, Dr. Bull also, knows full well that under exciting circumstances spermatorrhœa is apt to ensue, unless self-controled by strong chastening sense of moral duty. I have known several patients, professed religionists too, whose continence failed them. They continently commanded themselves for a certain while, but eventually "physiological laws" prevailed. In them I know prolonged continence brought on spermatorrhœa; in other words, incontinence. Perhaps Dr. B. means to maintain the sophism, that while continent there is no incontinence, but such tergiversation is beneath any honest reasoner's purpose. The question lies in the abuse of those physiological laws. Such abuse being at variance with morals, induced the two previously-mentioned illuminators of mankind to advise matrimonial remedy. Lallemand and Dangerfield have only attested the correctness of such advice; hence the conclusion drawn by them could not be more "monstrous an abuse, leading to gross immorality," than the *distorted* inference of Dr. Bull. To deny the assertion of continence, in the sense used by Dr. D., as a cause of spermatorrhœa, Dr. B. has recourse to the opinion of Cruveilhier on spermatocele. The analogy is incomplete, inasmuch as the latter affection is alike the result of mechanical injury, repeated morbid excitement, and half-accomplished onanistic purposes, as well as of continence.

* See "Introduction to the Study and Practice of Midwifery," by Drs. W. and A. Campbell, 2nd edit., p. 142.

The second, "continence never injurious to the general health," or, in Cruveilhier's own words, "animal economy." Cruveilhier, in Dr. B.'s own quotation, asserts atrophied testes and impotence to be the result of prolonged continence. If this occurs in the athletæ, why not in the Romish clergy? Unfortunately for the priests this is a medical Bull, and not an edict from his holiness, the pontiff. Having the concurring testimonies of Galen, Haller, and others, as to the condition of the Olympic continents, it is only a fair sequence to infer similar results as prevalent among the Catholic clergy. Thus continence, by *his own showing*, is proved to inflict physical and physiological injuries on some of those who observe the sacristic vow, as well as on those who have abided by their resolution under opposite duties.

In a subsequent paragraph M. Cruveilhier is stated "never to have seen a single case in which continence produced any evil consequences on the animal economy." Assuredly atrophied testes and impotence are injurious results; at least, most people would morally think so.

The third; "the laws of physiology *never* at variance with those of morality." The *negation* of this dogma is proved by Dr. Bull himself in his quoted opinions of ancient and modern physiologists.

In conclusion, I would remind Dr. Bull he has got to *prove* that the "laws of physiology are not at variance with morality." I remain, Sir, your obedient servant,

W. W. Morgan.

January, 1844.

CONTINENCE NEVER THE CAUSE OF SPERMATORRHŒA.

REPLY OF DR. BULL TO HIS CRITICS.

To the Editor of The Lancet.

Sir,—My letter on the above subject, published in your Journal of Dec. 9th, appears to have called forth no less than three replies (pages 398, 399, 401), as different in character as could well be. Although none of them has led me to change my opinions, I feel that some notice is perhaps due to each, which, if you can afford space to insert in your columns, I will promise not to trespass there again on the same subject.

Dr. Dangerfield, in his reply, scarcely broaches the point at issue, but complains principally of two things: that I have imputed to him opinions which he does not hold, and have ranked his name with that of the celebrated Lallemand. With regard to his opinions, I quoted two passages from Lallemand, but to the latter only did I append, distinctly, Dr. Dangerfield's coincidence; and for my justification in doing so I refer him to the conclusion of his first paper, quoted by himself in the present one. Whether they do not both lead directly to the same immoral inference of which I complain and attempt to refute, I leave to deeper casuists than myself to decide; I am quite content, however, to disentangle Dr. Dangerfield's opinions, whatever they may be, from those of the Professor, and deal with the latter alone. If he conceives the courtesy of connecting them together to have been misplaced, I can have no objection to withdraw it. My own views of the subject remain unchanged; and so entirely were they directed, in my last letter, against those of Lallemand, that I have not to remove, with the simple coincidence of Dr. Dangerfield, one particle of the argument or authority adduced.

There is a considerable interval between puberty and marriage, which I, at least, had not overlooked; and Dr. Dangerfield's fears that I hold that holy ceremony to be an "immorality," are as causeless as his apprehension that I should again condescend to criticise, with or without attention, any productions of one who can regard as an attack upon character a simple contravention of opinions; and who, while inveighing against want of charity, truth, and intellect, in others, affords so shining an example of his own. Leaving, therefore, to Dr. Dangerfield the undisturbed credit of the next "mite" which he shall cast into the treasury of science, I here take my leave of him.

The letter of the next writer I turn to with pleasure; his gentlemanly tone and argumentative style demand notice of a very different discription. Mr. Chatto has, unfortunately, mistaken my meaning at the commencement, and has thus attributed to me an argument which I have not used, and which I fully agree with him in pronouncing "fallacious." If he will oblige me by a re-perusal of my remarks, I am sure he will admit this. He makes me say that it is "impossible that sexual intercourse should prevent spermatorrhœa;—whereas the true spirit of my argument was to the effect that continence alone was sufficient to prevent this disease, without the aid of sexual intercourse; that continence alone can never cause it; and that continence alone may even cure it in its earlier stages. I could not say that sexual intercourse, in moderation, will not prevent it, for, of course, it must, that being the natural function of the organ; but I will now add, what seems to have been latterly much overlooked, that sexual intercourse in excess will actually produce it. In proof of this, I have at the present time under my care an advanced case of this disease, for which not only the patient's candid admission, but also information from collateral sources, ascribe excessive sexual indulgence as the cause.

Mr. Chatto seems to admit, throughout his letter, that the laws of physiology and morality are naturally in accordance, but

that our artificial state of society disturbs this harmony. The laws of nature, it is true, will not "bend and twist" themselves altogether into a different channel, according to our artificial usages; but they admit of these, as is demonstrated by the great modifications so readily produced in their relative action to meet contingent necessities. This is especially the case with the laws of physiology, which are ever ready, by their action, to adapt all parts of the economy to the contingencies they may be subject to—exercise or repose, heat or cold, &c. &c. The more any part or organ of the body is exercised, the greater the increase both in size and function which is produced, and *vice versâ* (diminution or emaciation being the result of perfect repose); always supposing, however, that this exercise be not carried to a degree which is inconsistent with the natural actions of the part.

Mr. Chatto goes on to say, with reference to our present subject, that "promiscuous sexual intercourse is only the lesser of two evils which the unfortunate arrangements of society force upon us," &c., referring, I suppose, to onanism or self-pollution as the other evil, for he does not mention the second. Here I disagree with him altogether, and contend that by simple continence both these evils may be avoided; and I maintain, also, that neither the one or the other is necessary for the preservation of health, or the continuance of the natural secretions from the testicles. It is only perfect continence, both mental and bodily, that is followed by the emaciation of the testicles and gradual suppression of their secretion, which I have alluded to as the result of a physiological law, and often observed (Cruveilhier) amongst the priesthood of the Romish church. In the present state of society, where the intercourse between the sexes is so intimate and habitual, it is almost impossible so to control the propensities and regulate the desires that the testicles should not be stimulated to continued secretion. But it does not follow from this that promiscuous sexual intercourse, or onanism, should take place, provided the inclinations and desires be properly resisted. The system can relieve itself, and will do so, at intervals, when the vesiculæ become loaded, and most frequently, too, during sleep, without the individual being aware of it, although the usual amount of orgasm may take place. Most young men, who are ordinarily moral, will have observed this every few weeks, according to the circumstances in which they may be placed at the time. This relief, too, may be, and constantly is, repeated for years without an evil consequence whatever to the body, and without rendering in the least degree necessary any breach of the laws of morality.

Mr. Chatto notices the cadaverous and unhealthy aspect of many of the Romish priesthood; but is not this rather owing to the prolonged fasts and rigorous discipline to which they are subjected by religious obligations, than to the simple fact of continence. Here let me repeat that Cruveilhier, with all his pathological skill and experience, has never seen a single case in which continence has produced any evil consequences on the economy, and he has had an ample field for observation amongst them.

I for one, too, am utterly opposed to Mr. Chatto's opinion, that ill consequences are daily to be observed upon the health of women in consequence of forced celibacy, and which would be avoided by the system of early marriages—at the age of puberty—which he seems to recommend. There are various other and more important causes for their various ailments than continence, and gladly would I enter upon them on the present occasion if they were not too long and too complicated to be treated with any degree of satisfaction. Moreover, since Mr. Chatto has not dwelt upon it, in answer to him I am not called upon to do so.

The third letter I have, in great measure, anticipated in my observations on the last. Mr. Dudgeon cannot understand the axiom I would wish to establish, and I am sorry, for his sake, that I cannot put it in plainer terms. With the imperfections of human nature, and the doctrine of original sin, of course we have nothing to do. Principally, however, he quarrels with my expression "continence may cure, but cannot cause, spermatorrhœa," both of which conclusions he utterly denies, as a general rule. Continence cure spermatorrhœa! Aye, indeed, Mr. Dudgeon—indeed it will. Continence alone has effected many a cure in the earlier stages of the disease. Nay, more, Sir, continence, I assert, must take a part in every cure; and these deductions I have arrived at from actual observations, whatever you may think to the contrary. When, from habitual want of continence, the impotent, because exhausted, patient, as a last resource, feeling bitterly his error, endeavours at length to be continent, it is no wonder that he should find it fail. Continence will not cure confirmed irritability. That must first be reduced to enable the patient to be continent. Every cause of irritation must be removed, such as sexual intercourse, onanism, &c. &c., and the usual methods for reducing it resorted to most perseveringly, before it is possible for him to exercise it.

Here, I must be pardoned for making one brief digression, to condemn strongly the practice of many medical men of the present day who, when a case of confirmed spermatorrhœa presents itself, at once recommend sexual intercourse, though not unfrequently they get for answer that the patient is unable to effect it. If Lallemand be correct in ascribing it so generally to excessive irritability of the genital organs, of which there

seems to be little doubt, surely such a recommendation must be not only unscientific but worse than useless. Such advice I am inclined to think it is, that has so generally driven the treatment of such cases into the hands of empirics.

Mr. Dudgeon, however, approaches my views much more nearly than Mr. Chatto. He admits that "*many* can abstain from breaking this law of morality without having their health injured," and for this great concession I am, also, most willing to admit with him that some constitutions are naturally predisposed to this disease. The rock upon which he seems to me to have foundered is the extent of his credulity. But for this, we should hold the same opinions. Let me remind him that, in arriving at any conclusion as to the real cause of spermatorrhœa, it must never be overlooked that it is a disease of a class which, more than any other, renders the patient indisposed to assign the true one. If continence is allowed as a plea (which, I contend, it never can be, except with some great and evident constitutional peculiarity) it is not likely that the patient should refer it to incontinence. A person will rarely throw so great a reflection, even in confidence, on his moral character, if there be any loophole of escape, any way by which he may avoid it. No; I repeat, continence cannot cause it, and if Mr. Dudgeon reflects upon it I think he will soon admit this, and enter the ranks on the side of morality, for, if I am not mistaken, he has both candour and liberality enough to do so, as soon as he shall become convinced that it is the true one.

I have now, I trust, Mr. Editor, sufficiently aroused the attention of the profession to this subject, and having, I believe, answered all the arguments adduced by the gentlemen who honoured me by replying to my first letter; having endeavoured, to the best of my ability, to uphold physiology, and prevent the perversion of her admirable laws to the sanction of immorality and the encouragement of vice, I take leave of the subject, with the hope, however, that it may not rest here. May other and more able pens, whose directors have a greater inclination than I have for a public controversy, be exercised on this subject. They cannot be occupied more nobly, and I venture to assert that they cannot take firmer ground than when they shall attempt to maintain that THE LAWS OF PHYSIOLOGY ARE NEVER OPPOSED TO THOSE OF MORALITY; and, with reference to the present subject, that CONTINENCE CAN NEVER CAUSE SPERMATORRHŒA, OR BE PRODUCTIVE OF ANY OTHER EVIL CONSEQUENCES TO THE SYSTEM. I am, Sir, your most obedient servant,

HENRY BULL, M.D., Edin.,
M.R.C.S.L. & E., &c.

Hereford, Dec. 30, 1843.

P. S.—I do not see THE LANCET until a week after publication, therefore, on the present occasion, I have lost no time in writing the above letter, with the hope that you will be able to find room for it in your next NUMBER.

*** Our correspondent will, ere long, be enabled to see THE LANCET on his table at Hereford on the day that it is published in London.—ED. L.

RELIEF IN CASES OF LYSSA.

To the Editor.—Sir: Attracted by your editorial remarks at page 337, of THE LANCET, in connection with an interesting letter of Mr. Turner, in which he recommends a trial of tracheotomy in the early stage of hydrophobia, I beg to forward a communication I had written, and intended to send to one of the medical journals many months ago. It was withheld only from a friend jocosely remarking that I should certainly be elected to the council of the besieged town. More than ever satisfied that the suggestion is not unworthy of consideration, I now beg the favour of its insertion in your columns. Our scientific friends, the veterinarians, unfortunately cannot put it in practice, for their patients have no uvula. In consequence of the remarks of Mr. Youatt I am inclined to ask, Is it the presence of the uvula which makes the spasm of the glottis so much more marked in man than in the lower animals? I am, Sir, your obedient servant,

J. YEARSLEY.

Saville-row, Dec. 23, 1843.

"I am induced, by the recent prevalence of hydrophobia, to throw out a suggestion relative to the treatment of that terrible and fatal disease. The most efficacious remedial means in spasmodic disorders allied to hydrophobia are, undoubtedly, those which inflict a sudden shock upon the nervous system. Excessive spasmodic action about the throat is well known to be a most prominent, and, probably, fatal symptom of the disease. It is this which occasions the difficulty of swallowing, and the extreme agony at the sight of liquids, or the bare idea of drinking or deglutition. Now, as excision of the uvula possesses in some cases a remarkable influence in controlling spasmodic action of the vocal and respiratory organs, it has occurred to me that it would be useful to perform this simple operation, as a slight shock to the nervous system, in the early or formative stage of hydrophobic disease. I have long intended to put this proposal to the test, should a case pass under my notice; may I hope the publication of the present letter will lead to a trial of its efficacy earlier than might otherwise occur. With the exception of the unsuccessful experiment of Magendie, of producing in an hydrophobic

patient an artificial repletion by the injection of large quantities of water into the veins, I do not know of any surgical plan that has been tried since the proposal of Marochetti to puncture the cluster of small pustules said to be found under the tongue. Of late years there have been very few novel propositions for the relief of rabies, and as the most powerful medicines prove utterly inefficacious, it seems to me that a surgical operation, such as that I have mentioned, is deserving of performance. It certainly would entail no risk, and would have a local action upon the parts most implicated in this frightful malady. In favour of local remedies, I may instance a recent case at King's College Hospital, under Dr. Guy, on the whole one of the most successful ever published; where ice was applied unremittingly to the throat, with the effect of greatly moderating the convulsive struggles of the unfortunate patient.

"I do not wish this to be considered as more than a mere suggestion, resting entirely on my own responsibility, which I make with a hope that it may prove useful, but without any established data on which to rest an expectation of its success."

KING'S COLLEGE HOSPITAL.

CASES,

WITH CLINICAL OBSERVATIONS,

BY MR. FERGUSSON.

HARE-LIP.—OPERATIONS ON INFANTS.

Rebecca Watts, ætat. three weeks, has a hare-lip fissure on the left side of the mesial line, for which the parents (both free from such malformation) are anxious an operation should be performed.

Oct. 7, 1843. To-day the margins of the fissure were pared with a narrow sharp-pointed bistoury, and the surfaces were retained in apposition by means of two twisted sutures. The proceedings occupied a very brief space of time, there was little loss of blood, and the child seemed to experience no great annoyance or suffering. Soon after its removal from the theatre it fell asleep.

9. Needles removed this morning; union apparently complete.

21. Has been brought several times to the hospital since last report. For the first week the upper lip was considerably swollen, but the tumefaction has now gone, and the part appears in a most satisfactory condition. The cicatrix is scarcely observable, unless on a close inspection. The child appears in excellent health, and has taken the breast regularly since the operation was performed.

In some observations afterwards delivered on this case, it was stated by Mr. Fergusson that there were various interesting points connected with it worthy of the attention of the pupils; and he should advert to some of them, chiefly on the ground that the operation itself, being one of little magnitude, was apt to receive less of their consideration than others of a more imposing character, but which were, nevertheless, of rarer occurrence in ordinary practice, and possibly, too, occasionally of more questionable utility!

After describing the nature of the malformation, Mr. Fergusson referred to the advantages of an operation. Some infants, he said, were observed to suck with difficulty when the lip was thus split; but the propriety of performing an operation to remedy this defect was doubtful; for, possibly, the child thus treated might immediately after refuse the breast altogether. As regarded speech, such a question was of little consequence during the infancy of a patient; and, perhaps, at this period of life, and, indeed, in all after years, the chief inducement towards an operation had reference to personal appearance. On this score there was good reason for interference, as neither parents nor friends could have much gratification in looking upon a child thus conspicuously marked; and, therefore, it only remained a question as to the best period for the performance of an operation. His own opinion was that, under all circumstances, the best period was immediately after suckling, provided the patient appeared in good health, and did not seem to suffer from teething. Here, as in all other cases about to be subjected to operation, it was the duty of the surgeon to examine as to the state of the system generally, and such being satisfactory, he imagined that there was no safer opportunity than that referred to. Some, however, had urged that a later age should be selected, while others had chosen an earlier; of the two, he should prefer the latter, although, indeed, it seemed to him to be, in the generality of cases, more a matter of choice as to will or convenience, when the proceeding should be done; for, usually, it was a very safe operation at all periods of life. He gave preference to the early age, as the patient was then more manageable, and he stated that he had experienced most trouble with those about eight years old, as the struggles of the sufferers were then more difficult of control. At a later age, again, the parties were usually very desirous to submit to the surgeon, and had sufficient command over themselves to keep perfectly steady. In the case under notice he had recommended delay for a

mouth or two, but as the parents had resolved to have an operation performed at all hazards, he had complied with their wish, being satisfied himself that there was no unusual risk in this instance. The pupils had now seen the result, and it was interesting to observe that the child had taken the breast without any apparent suffering, even when the pins were in the lip. This could not be expected in all cases, but he had seen others of the same kind, and had once operated successfully on a double hare-lip under similar circumstances.

CROOKED TOES.—SUBCUTANEOUS DIVISION OF FLEXOR TENDONS.

Ann Hearns, ætat. 17, admitted Nov. 15, 1843. About five years ago, after having worn tight shoes, observed a weakness in her toes, and that she was not able to walk so well as usual. She then discovered that the second toe on each foot was beginning to turn downwards, and since then the bending has caused the point of each toe to dip so much that it seems lost between those on each side. For several years she has been obliged to wear large shoes, and has had considerable inconvenience in walking. During the last five weeks has been confined to bed, in consequence of inflammation in the affected toes. The inflammation has now nearly subsided, but the parts are very tender. The point of each toe is much below those on both sides, and on the upper aspect of the joint, between the proximal and middle phalanges, there is a sharp knuckle-like projection, rising above the level of the other toes, crowned with a large and painful hard corn. Has entered the hospital for the purpose of having the affected toes amputated. Ordered to remain in bed, to give the toes complete rest.

Nov. 21. To-day a very narrow sharp-pointed blade was passed flatwise between the skin on the lower part of each toe and the flexor tendons, beneath the middle of the proximal phalanx; the cutting edge was then directed towards the bone, and the tendons were cut across. Some fibres immediately beneath the skin, which seemed to prevent the extension of each toe, were also divided, care being taken to make no other opening in the skin than that where the blade had been introduced. The members could now be stretched with facility, but neither could be made so straight as the others. There was very little bleeding, and the patient did not appear to suffer much pain. A small thin piece of light wood, about two inches long and a quarter of an inch wide, was then applied to the plantar aspect of each toe, where it was kept by a narrow strip of adhesive plaster, which surrounded the parts with firmness sufficient to keep the toes extended. The splint was enveloped in a portion of lint, to prevent the skin being injured.

Dec. 1. The splints have been continued up to the present time, having been re-applied with fresh straps every second day. The left toe is pretty straight, but the right is still considerably distorted. The corn or projection on the latter has become inflamed, in consequence of the pressure of the plaster, and there has, consequently, been less force used to produce extension. A small poultice to be applied over the tender point, and the splint to be continued with moderate pressure.

16. Inflammation has gone off; has no complaint in the toes, which are now both quite straight; the corn on the left toe has disappeared, and that on the right is scarcely perceptible; can walk without pain or difficulty. Discharged, with a recommendation to wear a roomy shoe in future.

Mr. Fergusson stated his belief that such a distortion as the above was very common, more especially in the well-formed foot, where the second toe was somewhat longer than the great one, and he was inclined to suppose that a short shoe was its probable origin, the point of the toe being thereby forced backwards, and the slender member bent at the joint between the first and second phalanx. Thereafter the deformity was, perhaps, increased, by the action of both flexors and extensors; the former acting on the middle phalanx, and drawing downwards and backwards, the latter on the distal end of the first bone, and raising it so as to cause the prominence which was so conspicuous in such cases. It was not always that so much annoyance was experienced as in this example, for some made no complaint whatever, while others were satisfied with the upper-leather of the shoe being loose at this point, or, perhaps, slit open. Some, however, suffered so much that they did not hesitate to have the part removed altogether; but it was his opinion that such a proceeding as had been followed here might, in many examples, be productive of great benefit. He had now resorted to the practice on several occasions, and having obtained the most favourable results, had been induced to bring the present case under the notice of the pupils, as an example to encourage them to similar treatment in other instances. The contractions had been so rigid that the toes did not, at first, straighten so readily as might have been expected; yet, by the continued application of moderate force, this desired end had been accomplished, and it was much more satisfactory to succeed in this manner than by the mutilating process of amputation.

ROYAL FREE HOSPITAL,
GRAY'S-INN-ROAD.

MEDULLARY SARCOMA OF THE LOWER JAW. EXCISION OF THE DISEASED PORTION. FATAL HÆMORRHAGE.

H. Bowles, ætat 37, was admitted into this hospital on the 29th of October, under the care of Mr. Gay. Her statement was as follows:—In the month of June last the first bicuspid tooth on the left side of the jaw, was broken off in an attempt to extract it, and the fang allowed to remain in the socket. She suffered considerable pain in the jaw immediately afterwards, and had leeches applied to the part again and again, without receiving any material relief. In the course of a few weeks a small but very painful tumour began to grow in the situation of the mental foramen, causing a distressing sensation of numbness in the portion of jaw and in the teeth between it and the symphysis. At this time (August) she appeared as an out-patient of the hospital. The fang of the tooth was removed, but presented no peculiar indications of disease. Leeches were applied, her constitutional health attended to, opiates given, and applications of iodine, belladonna, and veratrine made to the part.

On the 30th of September she was brought to the hospital, suffering from a severe attack of laryngitis, which had almost suddenly attacked her. By means of warm baths, leeches, and two grains of calomel, with one-sixth of a grain of tartar-emetic, given every two hours, the symptoms gradually yielded; so that on the evening of the 1st of October, twenty-eight hours after her admission, the mouth having become affected by the mercury, the pulse was softened, the difficulty of respiration had ceased, and she fell into a calm and sweet sleep. From this time to the 11th her improvement was so rapid that she left the hospital on that day well, as regarded the laryngitis. She did not, however, observe any improvement during this treatment in the maxillary tumour; on the contrary, its increase in size appeared to be more rapid in consequence of the ptyalism, and the pain became so severe that on the 27th of October she was again admitted.

The size of the tumour had now become equal to that of a large walnut, and its increase was obviously along the ramus of the jaw, in each direction, as well as towards the skin. Leeches were again applied, and morphine given, but without relief, even *to the severity of the pain*. The rapidity of the growth became more marked, and a consultation of the medical officers was held, at which the propriety of an early operation for the excision of the disease was determined upon. The patient readily consented, and on the 1st of November Mr. Gay removed the portion of the bone from the second cuspid to the fourth molar tooth. A portion of the skin of the side of the cheek, which had become involved in the malignant mass, was necessarily cut away, and the edges of the wound were brought together by sutures. The process of healing was rapid, the ligatures (three) came away in the course of a few days, and on the 18th she left, having lost all her pain, and with but slight external evidence of the loss of so considerable a portion of her jaw-bone.

A week afterwards she had eaten her dinner, when slight cough came on, and she exclaimed that she was a dying woman; a large quantity of florid blood forced its way through the nostrils and mouth, and in a few seconds she was dead. No post-mortem examination could be obtained.

The tumour appeared to have commenced in the mental foramen, immediately beneath the socket of the extracted tooth, and to have involved, primarily, the periosteum of the jaw, *upon* which it spread, and from which it grew towards the skin. The growth could not be separated or identified from the periosteum, but, together with it, was separable, by slight force, from the jaw-bone. The bone itself was dense in structure, but had no other indications of having undergone any morbid change. The texture was microscopically examined by the experienced eye of Mr. Reynolds, who detected in it all the physical elements of the true encephaloid cancer, or medullary sarcoma, in that stage when the roundish formative corpuscles deposited beside the fibrous tissue predominate. There were, also, many caudated corpuscles in the tissue.

TREATMENT OF HERNIA HUMORALIS, OR GONORRHŒAL ORCHITIS, BY OPIUM.

George Cox, ætat. 29, admitted an out-patient under Mr. Gay, Oct. 29th. This man contracted a gonorrhœa nine weeks since. Three weeks ago the left testicle and cord swelled, became very tender to the touch, and exhibited the usual symptoms of gonorrhœal inflammation of that organ. Leeches were applied, and the usual remedies administered, without any decided benefit, the testicle being very considerably inflamed on the day of his application at the hospital. He was ordered a brisk purge of calomel and jalap, to be followed by fifteen minims of tincture of opium, in camphor julep, every four hours.

On the 16th he reappeared, and stated that the pain had been relieved immediately after the first dose of the opium, and that, as he continued the medicine, so the tenderness and swelling gradually subsided, until all traces of the disease were removed, excepting a small indolent tumour in the epididymis, which is commonly to be met with in these cases, and usually remains after every other symptom has disappeared. A slight mucous discharge from the urethra was

treated by a mild injection of sulphate of zinc, and by it entirely removed.

October 20.—John Quin, æt. 40, had had gonorrhœa a month. Violent symptoms of inflamed testicle and cord supervened yesterday. He was ordered four grains of calomel, with a strong black draught, and afterwards fifteen minims of tincture of opium every four hours.

23. No improvement.

26. The pain and swelling have entirely subsided. A slight uneasiness is felt in the cord, at the situation of the external ring, but so trifling that it would not have been noticed by the patient had not the parts been roughly handled, to show the *complete* efficacy of the remedy.

October 23.—W. Wilkinson, ætat. 23. Has had gonorrhœa five weeks. A week since the discharge ceased, and the testicle and cord of the left side became inflamed. Leeches were applied, but without apparent benefit, for, on presenting himself at the hospital, the testicle, epididymis, cord, and scrotum, were involved in severe inflammatory action; his pulse was quick, and tongue foul. Five grains of calomel, with twenty-five of jalap were first given; and twenty minims of tincture of opium, with an ounce and a half of camphor-water, every four hours afterwards. He returned on the 2nd of November, and reported that, by the treatment prescribed, all the symptoms of inflammation had been removed, and the parts had regained their healthy condition.

These three cases are taken at random from above a hundred on record (besides many more, of which no notes have been made), in which gonorrhœal inflammation of the testicle and its appendages have been *speedily* and *completely* cured by purgatives *and opium*. Latterly hyoscyamus has been substituted for the opium with equally marked effects; but it must be observed that the free and full operation of the previous purgative has been found essential, in order to secure the full advantage of the subsequent part of the treatment.

COLCHESTER HOSPITAL.

STARVATION FROM ENLARGEMENT OF THE GLANDS AT THE CARDIAC ORIFICE OF THE STOMACH.

In this singular case the enlargement of the glands in question prevented food from passing into the stomach, thereby inducing extreme marasmus and death.—Mrs. Kerrit, aged fifty-three, for twelve months, or longer, had felt a difficulty in deglutition, the sensation being as though the food were stopped in its passage to the stomach; when it returned it was streaked with blood. Under these circumstances she came into the hospital, where she remained two or three weeks, and although, by taking fluid aliment frequently and in small quantities, she occasionally rallied, yet, it appearing evident that no permanent benefit could be received, she was removed home, and died in about a week or ten days.

The liver was enlarged, and a portion of the edge of the left lobe adhered to the cardiac orifice of the stomach. There were two or three tubercles in the liver, each about an inch in diameter, and of the consistence of cartilage. The mucous membrane passing from the œsophagus through the cardiac orifice of the stomach was thickened, and for two or three inches into the stomach it appeared to be raised from the level of the healthy membrane, so as to define the extent of the altered tissue. The glands at the cardiac orifice were enlarged.

It seems that the solid articles of diet accumulated at the seat of the stricture caused by the thickened membrane and the enlarged glands, the œsophagæal muscular fibres being thereby neutralised in their actions. When the food arrived at the constriction it would not pass beyond, and as hypertrophied parts are increased in sensibility, the irritation would, I apprehend, produce a contraction of the œsophagæal muscular fibres, beginning at the stricture and proceeding upwards; hence the food was regurgitated by mouthfuls, as it was swallowed!

E. Williams, Physician.

REMARKABLE CONVERSION OF THE OMENTA TO A SEMI-CARTILAGINOUS SUBSTANCE.

The subject of this form of disease, aged fifty-six, dated the beginning of his illness from exposure to cold. It would seem that there was great hereditary tendency to disease, as four or five of his brothers had fallen victims to phthisis, lumbar abscess, or some form of scirrhus. The apparent symptoms gradually developed themselves, and terminated in the dissolution of the patient, in about three years from their commencement; inability to retain food on the stomach beyond an hour or two, with general dropsy, ushering in the latter period of his existence.

The stomach, at its cardiac orifice, was engorged with venous blood, in other respects it was healthy as far as the pylorus. Here it presented a thickened aspect, and would scarcely admit the extremity of a finger, being constricted by the conversion of the glands to a gristly substance, the mucous membrane being healthy.

On the external surface of the stomach, the gastro-hepatic omentum, extending from the transverse fissure of the liver to the lesser curvature of the stomach and to the upper part of the duodenum, was changed to a gristly or cartilaginous texture. The effect of such change was impediment to the circulation through the liver, which produced dropsy. The great or gastro-colic omentum

was similarly changed, seeming as if contracted on itself, and constricting the transverse colon; by thus preventing the contents of the large bowel passing outward, the ascending colon was necessarily loaded.

The posterior lobe of the liver adhered strongly to the diaphragm. Touching this unusual adhesion, I may observe that nearly three years before his death he was very ill, and vomited some quarts of opake and mucopuriform secretion; and, as the lungs presented no appearance of disease, I am of opinion that at such period an abscess formed in the liver, and emptied itself by the lungs.

E. WILLIAMS, Physician.

THE LANCET.

London, Saturday, January 6, 1844.

EVIDENCE already pours in upon us in proof that the feeling of our readers will soon become universally—nay, even enthusiastically—in favour of the enlargement and stamping of THE LANCET, and of our efforts to establish a new era in medicine, by enlisting into its service, in this country, *as a lecturer*, the able and accomplished Professor of Chemistry at the University of Giessen, whose course of instruction to the class which crowds his laboratory this winter, has been expressly framed and adapted for subsequent publication in the columns of THE LANCET. A first part of these lectures is already before us, in manuscript, revised and corrected for the press by LIEBIG himself.

It must not be imputed to us, that in enriching medicine in England with the contributions of this celebrated man, a native of another country, we are overlooking the claims of our own countrymen as discoverers and improvers of animal chemistry, in order unduly to advance those of a "foreigner." A friendly correspondent, however, seems to apprehend that we shall expose ourselves with some degree of truth to this imputation, adding, that he has heard it more than once remarked, for instance, that we have never paid a proper tribute of respect to Dr. PROUT, as a labourer in the British school.

Come, then; since the subject is before us, and this name has been raised, let us promptly review the question that is thus brought on the carpet.

At once we say that Dr. PROUT deserves the reputation that he possesses in the department of science to which his attention has chiefly been directed,—earned, too, before organic chemistry had attained, in general estimation, its present high position—like anatomy—in connection with practical medicine,—earned at a period when few persons engaged in the same pursuit, and when, in fact, the methods of analysis were rude and defective compared with those which are followed at the present moment. His fame rests upon his papers in the "Philosophical Transactions," and certain periodicals of the day. Many organic analyses, made by him, and some of his theoretical views, are there recorded,—all of them, now, matters for the *historian* of medical science, from whose pen, no doubt, Dr. PROUT will meet with a just measure of renown. By this time, however, his main researches have been superseded by others, based upon minuter analyses, or on demonstrations which have been carried out with a much nearer approach to mathematical certainty.

His more recent productions are the "Bridgewater Treatise," and the third and fourth editions of his work "On the Nature and Treatment of Stomach and Renal Affections." These may be presumed to contain the theoretical and practical results of his earlier analytical labours. Many individuals hold Dr. PROUT to be the first of British organic chemists; but several of the doctrines that he espouses are opposed to those which are now taught in the continental schools that possess the highest repute. A reference to some of these discrepancies will set us right with regard to the point that we are now venturing to discuss. Our attention, moreover, is specially called to it by the publication of a new edition of his well-known work. Take we up, then, that volume for our purpose.

In the first place, Dr. PROUT has retained in the text of his last edition his own account of the process of digestion in the stomach, without taking proper cognizance therein of the digestive principle *pepsin*. This, and the lucid views of SCHWANN and MULLER, are mentioned in a foot-note only. Yet no doubt can possibly exist in the mind of any man of science as to the existence and the agency of such a principle. Therefore, as respects this important process, Dr. PROUT lags far behind his contemporaries of the present day.

He observes, of albuminous substances, that their exact composition cannot be stated, and adheres to the older method of regarding those substances as compounds of the four organic elements, with minute portions of certain "incidental" mineral substances. MULDER's discovery of *proteine* is entirely neglected; yet its existence, and the important part that it plays in organic metamorphoses, is admitted on all hands.

We are not certain whether the use of the term *incidental* originated with BERZELIUS or Dr. PROUT, but the latter employs it in a very questionable manner. In the vulgar tongue it means a *casualty*, or something happening *by chance*. Now, the sulphur and the phosphorus of fibrine, or albumen, are understood to be as essential, and as little incidental, to the composition of those substances, as is oxygen or nitrogen. We are aware that credit is due to Dr. PROUT for having,—at an early period, when chemists regarded the saline and earthy residues of their analyses as so much useless refuse, contended against the error, and maintained that those constituents played unknown but important parts in the living economy. Yet, after other chemists have most cleverly demonstrated the truth of this, why retain a term which is calculated to induce a misconception in the minds of students and others as to the true interpretation of numerous chemical facts?

Dr. PROUT differs from LIEBIG, and the continental schools, in paying much less regard to the progressive transpositions of the elements of organic compounds in living systems. It is true that, in the case of sugar, Dr. PROUT admits the possibility of its passing through the same series of changes *within* the body as *without*, during its conversion into alcohol. He mentions other "conversions," also,—such, for instance, as that of saccharine into oleaginous principles, but he does not appear to admit that the constituents of the bile, the urea, and the other compounds belonging to the urine, or the carbonic-acid gas exhaled by the lungs, are so many substances developed by *progressive*, as contra-distinguished from *abrupt*, changes in the forms of matter. While the signification of the term "fermentation" is the same as heretofore, its application has been greatly extended by the disciples of the LIEBIG school, not only to the digestive processes, but to nearly all the molecular phenomena of life. Every change that takes place in the composition of matters, be it in building up or in destroying the vital structures, is liable to prove one of a series. Dr. PROUT pays little respect to this view of the case.

On the contrary, he questions the accuracy of the analyses upon which these views are founded. Thus, as respects xanthic or lithic oxide, WÖHLER and LIEBIG have shown that it differs from lithic acid in containing one equivalent less of oxygen. To this two objections are taken, neither of which is valid. In the first place, Dr. PROUT doubts the capability of chemists "of unequivocally de-"ciding about the presence or absence of one "proportion of hydrogen, or even of oxygen, "in a complicated body." Secondly, he doubts the fact, because the combining weight of carbon has been taken at 6.12 instead of 6. But the proportion of carbon to oxygen is as 10 eq. to 4 eq. in the one compound, and as 10 to 6 in the other; or, 39.86 carbon to 20.82 oxygen in lithic oxide, and 36.00 carbon to 28.27 oxygen in lithic acid, and, accordingly, the difference in the equivalent number is too small to affect the conclusion.

But the doctrines of Dr. PROUT and LIEBI

are, beyond measure, wide apart on the subject of nutrition. Dr. PROUT calculates the nutritive powers of alimentary principles by the quantity of carbon that they contain. LIEBIG regards the carbon as an element of respiration, measuring the nutritive powers by the quantity of azote, or of azotised compounds. By a chain of evidence, irrefragable, as it appears at present, he has concluded that all the organic nitrogenised constituents of the body, how different soever they may be in composition, are derived from proteine. Dr. PROUT teaches that fat admits of the highest degree of organisation of which matter is susceptible, and that it is most completely and essentially nutrient; LIEBIG believes that its elements serve only as fuel for the production of animal heat. The former regards gelatine as the least perfect kind of albuminous matter; the latter looks upon it as a substance of a totally different class.

The same discrepancies are observable in their pathological chemistry. Both regard lactic acid in the stomach as the product of disease, but LIEBIG refers it, at all times, to the food; whereas PROUT, assuming that lactates exist in the blood, thinks it is most frequently derived from the blood itself. In diabetes, the former states that the starch of the food is converted into grape-sugar, and expelled from the body without further change. Dr. PROUT admits this, where he says that in diabetic affections the reducing function of the stomach seems, in some instances, to be almost morbidly active, and farinaceous matters are reduced to the condition of low saccharine matter, the converting function being, at the same time, destroyed; but he believes that the source of the sugar in this disease is mainly the secondary assimilating processes,—that it is formed from the gelatinous, the albuminous, and even the oleaginous tissues. Whatever truth there may be in the opinion that some of these tissues yield the sugar appearing in diabetes, that opinion, in Dr. PROUT's work, is supported almost entirely by a very imperfect and questionable analogy. We are told that lithate of ammonia occurs in the urine, as a consequence of bad digestion, when too large a quantity of crude food has been taken into the stomach, and that the same salt is also excreted in acute diseases, when no food whatever has been received into the stomach, its source being the albuminous tissues. From this we are taught to infer that the sugar in diabetes has a like double origin; but we shall presently point out why it is, as appears to us that Dr. PROUT does not seem to be aware that there is no analogy in the two cases.

It might be inferred, from a perusal of Dr. PROUT's pages on urea and lithic acid, or its compounds, that he is totally unacquainted with the action of OXYGEN—of vital air—upon the animal tissues. He does not refer to it, that we can find, in one instance. WÖHLER and LIEBIG have beautifully developed the results of this agency. Dr. PROUT speaks of lithic acid as being principally derived from albuminous textures, and of urea as principally derived from gelatinous textures; and, in his last edition, it is in a foot-note only that he makes any allusion to the relations in composition between these and allied substances. LIEBIG, on the contrary, traces lithic acid from its origin in the blood, or the muscular tissue, by the action of oxygen on proteine. He develops the products of the further oxidation of proteine, and of lithic acid, alloxan, oxalic acid, carbonic acid, urea, &c. He shows that uric acid is secreted abundantly in many animal systems where oxidation is incomplete; that in the physiological state of the human system it is replaced by urea; that here it presents itself as gravel and calculus, when there is deficient oxidation; that it yields the intermediate compound, oxalic acid, or the mulberry calculus, with urea, when the quantity of oxygen is somewhat increased; urea and carbonic acid only occurring where there is a full supply. LIEBIG thus establishes a series of relations, of the highest practical and theoretical importance, between the

renal secretion and the functions of respiration, digestion, and muscular motion.

We will now show how, as appears on the face of Dr. PROUT's work, it happens that so great a difference should have arisen between himself and his admirers on the one part, and his continental cotemporaries on the other, in their views respecting the animal economy. Dr. PROUT has "purposely "omitted the formulæ now so much in "fashion among chemists," not only because he considers them "clumsy and unphilosophical as conventional expedients," but because he is satisfied "that very few, "if any of them, represent the true compo-"sition of organised substances." In other words, our author has not kept pace with his science. It is admitted on all hands that the greatest advantages have accrued from the use of these formulæ, and we hope Dr. PROUT will live to see new discoveries resulting from the accuracy of research with which their employment has been necessarily attended; we must also, in candour, affirm, that had Dr. PROUT himself paid more regard to them, we verily believe he would never have placed reliance upon several false analogies, and would have suppressed from the world some hypotheses which are unsubstantiated either by facts or arguments; for—notwithstanding that we are told in the preface to the fourth edition on Stomach and Renal Diseases, that the work is practical, and precludes all controversy—we will venture to say that there is no "standard" work of the present day which contains more hypothesis, or more matter of controversy.

That sugar is converted into albumen in primary digestion;—that albumen is converted, during the nutritive process, into gelatine by the elimination of carbon;—that urea and lactic acid are complementary to each other, being derived chiefly from the gelatinous tissues during their "destructive assimilation;"—that the assimilating organs can decompose the reputed elements of matter, and form azote and carbon;—that the stomach is possessed of a vitalising power;—that the use of tobacco produces the development of a poisonous principle analogous to oxalic acid;—that snuff induces malignant disease of the stomach and liver—are, to say the best of them, so many weakly-substantiated hypotheses. Dr. PROUT's pathological notions,—those, especially, which relate to the state of the kidneys in albuminuria,—have the appearance of closet speculations, and are diametrically opposed to the opinions of the soundest and most experienced men of the day. His views on diabetes are hypothetical and controversial; the theory he adopts excludes many of the facts of the case, and even where there is every reason to believe him to be correct, the value of his observations is much lessened by his total disregard of numerical and statistical details.

We desire not to be misunderstood. Dr. PROUT deserves, and will receive, honour for his earlier labours in the field of organic chemistry. His analyses were unquestionably, for the most part, accurate, and he made the most of the facts and materials at his command in applying chemistry to the explanation of the phenomena of life. Physiology is indebted to him for determining the nature of the acid of the gastric-juice, and for describing the staminal alimentary principles of milk and other articles of food. In these pursuits he evinced great zeal, at a time when chemical doctrines were little in vogue among the professors of medicine; and many of his suggestions, in reference to the application of chemistry to the science and art of healing, might be reproduced, and would hold good, at the present day. The continental school has, no doubt, taken up the inquiry where Dr. PROUT may be said to have left it, and our principal reason for thus particularly dwelling on the subject is, that although the authority of names has, in a measure, lost its weight, we know that Dr. PROUT's name and authority exercises an influence that is detrimental to the cause of science in this country.

In last week's LANCET another great step in advance is recorded,—the discovery by

MULDER, "the illustrious Dutchman," of the oxides of proteine, and of some of their remarkable properties. How does it happen that none of these discoveries are now being made by British chemico-physiologists? We could name a dozen, *seriatim*, the honour of which is due to foreign professors. Time was when our schools stood foremost in the ranks for originality and discovery, but that time seems to have passed away. There is no difficulty in showing where the fault lies. It is to be found in the mode of teaching chemistry in this country, which, both as an ART and a *science*, requires a procedure very different from that hitherto pursued—in the ignorance or carelessness of examiners, whose eyes do not yet appear to be open to its importance—in the supineness of its professors, who have never seriously bethought themselves of really teaching it to young men; and perhaps, among the rest, to the authority of those who, having earned a reputation for themselves, cast unfounded doubts upon the labours of others, neglect and repudiate, without sufficient cause, the methods followed by their competitors, and deny them that honour to which they are justly entitled by their discoveries. We regret to find Dr. PROUT in this category.

TENDENCY TO INSANITY AT CHILDBIRTH.

PARTURITION and delivery are not uncommon causes of temporary insanity. In accusation of infanticide against the mother temporary insanity may, therefore, be pleaded in defence, and in some instances be a perfectly valid ground of exculpation, the more especially as infanticidal mania is one of the most common forms in which temporary insanity manifests itself in connection with delivery. Rabbits, cats, bitches, swine, and some birds are peculiarly subject to have the maternal instinct to protect, converted into a furious passion to destroy their offspring. (See Pierquin, "De la Folie des Animaux," tom. ii.) Even in natural labour, especially in first births, the mental faculties are frequently affected. Moral or physical causes, either singly or combined, convert this unsettled state of mind into actual mania; and both causes are peculiarly apt to be in operation in unmarried mothers, against whom charges of infanticide are most common.—*Dr. J. R. Cormack.*

A Supplementary Report on the Results of a Special Inquiry into the Practice of Interment in Towns. Made at the request of her Majesty's Principal Secretary of State for the Home Department, by EDWIN CHADWICK, Esq., Barrister-at-Law. London. 1843. 8vo. pp. 279.

THIS is a supplement to the General Sanitary Report of Mr. Chadwick. The report is pretty well known to our readers. It contained, however, no reference to the graveyards of London and the larger towns. This Mr. Chadwick considered to be a defect, which he has sought to supply by the supplement. He had the report of a parliamentary committee to work upon, adding to it a publication on the subject by Mr. Walker. He also applied to the clergy, the dissenting ministers, several physiologists, numerous undertakers, and secretaries of benefit and burial societies, and some foreign governments; in fact, to every source of information which he could make available to his object. The result is the present compilation, which displays great industry, and evidence of much analytical power over scattered and heterogenous materials. Amongst other things that we are happy to discern in it is, a juster appreciation of the merits and utility of the medical profession than he before deserved the credit of entertaining. In addition to a complete review of the practice of interment in towns, a plan is laid down for the appointment of officers of public health, for visiting every dead body; ascertaining, as nearly as possible, the cause of death; giving notice to and assisting the coroner, if necessary; offering their services to the survivors in directing the funeral arrangements, and inspecting cemeteries. The scale of payment for this complex, arduous, and dangerous service, which must continually expose the officer to infection, and demand of him incessant labour, for death gives no holidays,—the scale of salary is proposed to be the same as for staff-surgeons. In an appendix, Mr. Chadwick gives the public funereal regulations that are in force at Frankfort and Munich; several interesting communications from union surgeons, illustrative of the various points discussed in the report; and other documents, similarly useful and apposite. The most interesting of these is a return of the average ages at which deaths and funerals occurred during the year 1839, among the several classes of society resident

in the superintendent-registrars' districts of the metropolis, wherein various general facts and arguments are stated which invalidate the all but unanimous opinion of statistical writers that the number living in proportion to each death, and the average age *at* death—or, in other words, the average duration of life,—are equivalent; as, for example, 1 in 32 being the ratio of mortality, 32 expresses the average age at death. This essay in vital statistics (for such the article termed "returns" really is) deserves careful perusal. Indeed, the whole volume merits attention, were it only for the impression that it is calculated to create in favour of the value and importance of medical science to the community.

COLCHESTER MEDICAL SOCIETY.

To the Editor of THE LANCET.

SIR,—As the letter of Dr. Williams, in your last number (page 454), is calculated to produce a very erroneous impression as to the grounds upon which the Colchester Medical Society came to a resolution touching his conduct, I shall feel obliged if you will give insertion in your next to the following statement of facts:—

At an ordinary meeting of the society, held on October 31, 1843 (from which Dr. Williams was absent), certain matters were mentioned relative to his conduct as a professional man. In consequence of his absence they were not then entertained, but it was agreed to call a special meeting at an early period, which was held on December 6th, at which the following charges were brought forward against Dr. Williams:—

First. For contracting with a party for yearly attendance and payment.

This charge was considered proved, by the production of an agreement, of which the following is a copy:—

"Memorandum.

"Laker, William, wishes me to attend him for three guineas a year, payable quarterly; should my attendance be necessary at his residence, it is to be at the rate of half-a-guinea the week; the year to commence on the 7th day of October, 1843.

(Signed)

"E. WILLIAMS, WILLIAM LAKER."

Of this contract two copies were made, one of which was retained by the Doctor, and the other by William Laker. Besides the production of this contract Dr. Williams, after repeatedly asserting that it was an *individual* departure from his usual practice, acknowledged that during his residence in Colchester he had adopted the plan in *six* or *seven* other instances.

Second. For contracting with one druggist for dispensing his prescriptions, and attempting to do so with another.

The proof of this charge was, the production of a letter from Mr. Edwards to the Doctor (which he has published in your last week's number), which admits a contract; also Dr. Williams' refusal to deny that he derived pecuniary advantage from that contract, by the production of prescriptions, written on paper, on the back of which he had Mr. Edwards' name and address printed; as well as by a statement, made by Mr. Hitchcock, another druggist, that Dr. Williams had sent for him, and offered to enter into an arrangement with him upon Mr. Edwards' retirement from business, which Mr. H. refused to do.

Third. For prescribing medicines which only his own druggist could dispense, and making such medicines secret.

This was proved by the production of a prescription containing "vin. ferri aromat." (the formula for which is to be found in Braithwaite's "Retrospect." When Dr. W. gave this prescription to his patient (not gratuitously) he told him it could only be prepared at Edwards'. The patient, a customer of Mr. Hitchcock, took it to him, who, not knowing anything of that preparation, was unable to dispense it. Upon a repetition of this prescription, and a remonstrance on the part of his patient, Dr. Williams called on Mr. Hitchcock, and told him that he could purchase the "vinum" at Edwards', but that the recipe was a *private* one of his own, so much so, that he did not even permit Edwards to know it, but prepared it at his own house, and supplied Edwards with it himself. The same was proved in another case, also, when the patient was a customer of a third druggist (Mr. Payne), who received a similar statement from the Doctor, though in this instance Mr. P. called on Dr. Williams.

Fourth. For taking a fee from an out-patient at the hospital, for attendance on that patient at his house.

In proof of this charge it was shown that on Oct. 9, 1843, an out-patient at the Essex and Colchester Hospital, named Boyden, residing at Leyden, sent his daughter to Dr. Williams, at the hospital, requesting that he would go and see him, as he was too ill to go to the Doctor. Dr. W. replied that if he went he must have a fee, which would be half-a-guinea. He went, and took his fee, and the prescription was dispensed at the hospital.

Dr. Williams' own case-book corroborated this charge, so far as taking the fee was concerned; and reference to the house-surgeon proved that the prescription was dispensed at the hospital, Boyden's name being on the books as an out-patient.

So much for the facts; now (to use the Doctor's words) "for the resolution."

There were thirteen members present, nine of whom voted for it. Two (Dr. Chambers, the other physician, and Mr. Baker, the house-surgeon at the hospital,) were neutral. I voted against it, as also did Dr. Williams himself, exclaiming, "I shall not vote against myself."

I offer no remark, but leave the profession to judge whether the Colchester Medical Society has acted at all harshly in pronouncing such acts unprofessional. I am, Sir, your obedient servant,

SAMUEL A. PHILBRICK,
Sec. to Colchester Medical Society.
Colchester, Jan. 3, 1844.

BRITISH MEDICAL JOURNALS.

BOTH wheat and tares are to be found in the fields of our contemporaries, and, accordingly, as leisure and space have permitted, we have gleaned and weeded amongst them for many years, gathering corn to the universal garner, and cutting down docks for the general good. Latterly, we have performed these tasks in a discursive manner, making a sheaf here in our pages, and a sheaf there, and turning up a white-bottomed thistle to die in the sun, now and then, only, as the humour took us, or the spud was in hand. But the labour may be performed more agreeably to ourselves, and conveniently for our legions of subscribers, if we collect the produce of our sickle, and present it to them within a stated compass every week. For the future, then, under some such general title as we have adopted on the present occasion, or in some other equally suitable way, our extracts, analyses, and criticisms of contemporaneous contributions to medicine will be thus presented to the readers of THE LANCET. We plunge, now, *in medias res*, and give the first worthy extracts that come to hand:—

"RACHITIS.—The bones in rachitis are chiefly characterised by the small quantity of fixed salts they contain. Davy found the following proportions of earthy salts in rickety bones:—

In the femur	37.8
In the spinous processes .	40.7
In the ribs	40.8
In the tibia.....	26.0
In the parietal bone	27.1

"Lehmann (Physiologisch. Chemie. Bd. 1, p. 111,) found in the tibia of a rickety child 33.9 of phosphate of lime.

"Marchand (Journal f. Pract. Chemie., October, 1842,) examined several bones of a child who died of rickets; the result of these inquiries was, that the spinal vertebræ had lost most, the sternum least, of the fixed salts.

"Marchand found the cartilage of the rickety bone so essentially changed, that on being boiled it yielded a fluid containing neither gluten nor chondrine.

"Marchand, on examining the urine of the child, whose bones were subjected to experiment by him, as we have just now seen, found, a short time before death, five or six times more of earthy phosphates than are found in healthy urine. Berzelius showed how extremely easily the phosphate of lime is dissolved by lactic acid. As in scrofula and in scrofulous bones, an excessive formation of acid is generated in the primæ viæ, it is not improbable that a greater quantity of acid reaches the circulation, and the proximate cause of the solution of the bone-earth emanates from the bones. So long as the alkaline salts of the blood are sufficient to satiate the excess of acid, so long are the bones preserved in their healthy state; but if this is not the case, their bone-earth becomes dissolved. Marchand very justly animadverts on the absurd treatment of rickety children with madder. The natural mode of treatment should strive to meet the cause of the excessive generation of acid by suitable diet and antacids, and in the next place all those alimentary substances which are capable of forming lactic acid, as sugar, starch, and gum, must be avoided."—*Med. Chirur. Review*, Jan.

CRITICAL PERIODS.—"Muret says that the epoch between the 40th and 50th years of female life is not attended with greater danger than that between the 20th and 30th years; and M. Saucerotte has shown, by numerous statistical tables, that the mortality is actually higher between the 30th and 40th than between the 40th and 50th years of a woman's life.

"Lastly, M. Chateauneuf has proved that the epoch of life between the 40th and 50th years is really more critical for men than for women."—*Idem.*

CURABILITY OF PHTHISIS.—"M. Fournet alludes to his having met with, in the course of one year, no fewer than 14 cases of confirmed phthisis that were cured; besides 10 other cases, in which dissection revealed the traces of caverns that had become perfectly healed.

"He goes on to remark, that 'these 14 cases of phthisis, cured in the living subject, have proved to me—

"'1. That certain persons, who have exhibited the most decided symptoms of the disease, in its most advanced stage, may yet be restored to excellent health.

"'2. That, if the general state is satisfactory in these individuals, and does not occasionally bear the evidence in some manner of the accidents of their past life, the local condition is very different, and always reveals the presence of alterations, more or less extensive.

"'3. That even hereditary phthisis, in its most advanced stage, is susceptible of cure; although such an occurrence is certainly much more rare than in cases of the accidental disease.

"'4. That phthisical patients, who have been treated by very various kinds of remedies, or who have been left entirely to the resources of the natural powers of their economy, seem to have recovered in about the same proportion; and, therefore, that nature generally 'fait tous les frais' of the cure of the disease."

"He concludes his remarks with the following sentence:—'The capital fact which seems to spring from these inquiries is, that tuberculous disease is not, like cancer, essentially incurable; on the contrary, that it is often curable, and that its extreme and most disheartening fatality is referrible rather to the circumstance of its being seated in one of the vital organs of the system, and to its tendency to frequent relapses, than to its primary and essential nature.'"—*Idem.*

TOBACCO.—"The constituents of tobacco-smoke and of the products obtained by the dry distillation of tobacco, are *a peculiar empyreumatic oil, butyric acid, carbonic acid, ammonia, paraffine, empyreumatic resin, water,* a little *acetic acid, carbonic oxide,* and *carburetted hydrogen.* It is deserving of observation, that no creosote is formed in this distillation, which will account for the circumstance that tobacco-smoke is less irritating to the eyes than wood-smoke."—*Pharmaceutical Journal.*

DROPSY AFTER SCARLET FEVER.—Mr. Lipscomb, in a paper in the "Medical Gazette," Dec. 29, on this subject, "recommends that in all cases of recovery from scarlet fever the urine be frequently and accurately tested (by heat and nitric acid) that the slightest trace of albumen may be detected as early as possible." This recommendation is in accordance with one which we have for a long time urged on the subject. The presence of albumen in the urine may be detected some time before the occurrence of dropsy. If *then* judicious remedies are applied this latter affection may be altogether prevented. The remedies most applicable are such as determine to the cutaneous surface; warm baths, warm clothing, &c., together with local bleeding and saline diuretics.

COD-LIVER OIL.—We have rarely seen any beneficial results from its use, a fact which may be explained by the following statement of Mr. Chalk, who, in reporting very favourably of its effects in scrofula and other diseases, says, on the authority of Mr. Wright, that there are "oils of various qualities, some of them quite inert. The liver of the milter cod yields the strongest oil, and darker in colour than that of the spawner. The two specimens now by me are exceedingly pure, nearly inodorous, and of a crystalline clearness. One is of a brown colour (the milter), the other of a light yellow (the spawner); the specific gravity of the former is ·921·5, temp. 61; that of the latter, ·919, temp. 61. The quality of the oil is considerably impaired when a high temperature is employed in the manipulation."

THE AVERAGE NUMBER OF DEATHS IN CAPITAL OPERATIONS.—A more appropriate title for this essay would have been "The Liverpool Infirmary, its advantages over the Northern Hospital." The paper contains a vast amount of egotism, and a few statistical facts which may be of use to such of our readers as feel interested in them. It may be found in the last number of the "Medical Gazette."

DR. FREDERICK BIRD records a successful case of "diseased ovarium" in which the tumour was removed by the "*minor*" incision. The patient's age was twenty-one; the case does not present any points of particular interest. The doctor, however, concludes his remarks with the following observations, the force of which we trust will not be altogether lost on himself:—"To record faithfully, and with equal publicity, successful and unsuccessful cases is, in the present state of our knowledge, a moral obligation, from the full performance of which no honourable mind would shrink."

CARCINOMA.—"In the scirrhoid varieties of carcinoma, the prevailing or predominant figures in the ultimate cell are the orbicular or circular. In all the sub-varieties, the cells are more or less completely reducible to this conformation; the stone-like density of scirrhus is due to the fibrous stroma, in the meshes of which the true cancerous cells are contained. These intersecting stromatous fibres, according to the opinion of the best authors, cannot be considered a part of the malignant growth; they contend that they consist of the proper structure of the part, thickened, tightened, and otherwise altered by the local agency of the disease."—*Provin. Journal.*

ENTRANCE OF AIR INTO THE VEINS.—In an essay on this subject Mr. Erichsen concludes as follows:—

"1. That the primary arrest of the circulation takes place in the capillaries of the lungs, or in the terminal branches of the pulmonary artery in consequence of inability in the right ventricle to overcome the mechanical obstacle presented by air-bubbles in these vessels.

"2. That respiration and animal life cease in consequence of a deficient supply of arterial blood to the central organs of the nervous system.

"3. That as air enters the veins in quantity, in force, and in rapidity, propor-

tioned to the depth of the inspirations, the best mode of preventing the occurrence of the accident, or, at all events, of lessening its probable fatality, would be, in all operations about the dangerous region,—the root of the neck and summit of the thorax,—to bandage the chest tightly with broad flannel rollers or laced napkins, so as to prevent deep gasping inspirations, and to keep the breathing as shallow as possible, consistently with the comfort of the patient.

"4. If air have already gained admission, prevent its further entry by compressing, or, if possible, ligaturing the wounded vein by which it had entered.

"5. Keep up a due supply of blood to the brain and central organs of the nervous system, by placing the patient in a recumbent position, and by compressing his axillary and femoral arteries.

"6. Maintain the action of the heart, by artificial respiration and friction on the precordial region, until the obstruction in the capillaries can be overcome or removed.

"7. Remove, if possible, the obstructions in the capillaries of the lungs by artificial respiration.

"8. If the patient survive the immediate effects of the accident, guard against the supervention of pneumonia or bronchitis."—*Edin. Med. and Surg. Journal*, Jan.

THE ALLEGED INFECUNDITY OF FEMALES BORN CO-TWINS WITH MALES.—Dr. Simpson, in a very interesting paper on this subject, concludes—

"1. That, in the human subject, females born co-twins with males, are, when married, as likely to have children as any other females belonging to the general community.

"2. That, when they are married and become mothers, they are, in respect to the number of their children, as productive as other females.

"3. That the same law of the fecundity of the female in opposite sexed twins seems to hold good among all our uniparous domestic animals, with the *exception* of the cow alone."—*Idem.*

SPINA BIFIDA. — At a meeting of the Surgical Society of Ireland, Dr. Mitchell exhibited a specimen of this malformation, and stated that "the person who gave birth to it had been first seen by one of the pupils of the South-Eastern Lying-in Hospital, and when he first saw the fœtus he took it for a case of spina bifida anterior, from the strong resemblance which it bore to Cruveilhier's specimen of that affection. It proved, however, to be an extraordinarily large specimen of spina bifida posterior, situated in a very unusual place, inasmuch as that it extended from the third cervical to the fifth dorsal vertebra, the spinous processes of all the intermediate vertebræ being deficient. The bones of the head had been also arrested in their development, and their positions much altered—the petrous portion of the temporal bone occupying the situation of the occipital. The brain was enclosed in a cyst, having a division down the centre, and communicating with sac that covered the spine. The thoracic viscera were all natural, with the exception of the heart, which was of a disproportionate size. The abdominal viscera were quite normal; and one of the most striking facts in connection with the monster was, that the platysma myoides was developed to a most extraordinary degree. The creature lived for about twenty minutes, and from the circumstance of the eyes being very prominent—he might say protruding—and there being a remarkable projection of the upper jaw, leaving the sacs of the teeth fully exposed, it presented a most hideous aspect—so much so, that it considerably frightened the nurse in attendance. It was not his intention to enter on the question of the formation of such monsters, or in what way the mind of the parent could act upon the fœtus in utero under powerful impressions. He would merely observe, that the woman attributed the cause of her having that malformation to the fact of a cat having been thrown on her back when about two months pregnant."

In the discussion which followed on the influence of external circumstances on the fœtus in utero, Dr. Houston related "the case of the lady of a dispensary surgeon who was still living. Her husband had been engaged one day in an operation on a man who had received an injury on one of his hands by which either two or three of his fingers had been chopped off. While in the act of dressing the man's hand, his lady happened to have some message to deliver to him, and looked into the room, when she saw the bloody hand with the fingers wanting. She was at the time in an early stage of pregnancy, and when delivered, her child was born with the corresponding fingers missing, which the individual referred to had lost. The lady had had children previously who were all regularly formed; but every child she has since had has been born with the same defect. The case made a strong impression on the mind, that mental affections of the mother, while *enceinte*, produced an effect on the fœtus in the womb."—*Dub. Med. Press*, Dec. 27.

NITRATE OF POTASS IN ASTHMA.—"A correspondent of the 'New York Med. Gaz.' says that he has derived essential benefit from using the following remedy in severe attacks of asthma, and has prescribed it for several patients with equal success:—Immerse thick porous paper in a saturated solution of nitrate of potass, or common saltpetre, and hang it up to dry. At the approach of a paroxysm, inhale the vapour by burning it in the room, or smoking it in a tobacco-pipe. The writer says, he 'is ac-

quainted with several asthmatics who are unable to breathe in a recumbent position until their sleeping apartment has been filled with the above vapour.' "—*Idem.*

WORMS IN THE SCROTUM OF THE HORSE.—Mr. King, of Stanmore, says, "by some chance, handling *one* of the testicles of a yearling after removal (and I have often since wished that notice had been taken of the other), two worms were discovered lying between the epididymis and the testicle, about an inch and a half long, looking very like young lumbrici; they were preserved, and are to be seen now, I believe, in the Museum of the Veterinary College. They appeared to have been stationary, and the testicle exhibited every usual characteristic of a healthy gland."—*Veterinarian*, Jan.

CALCULUS.—"Table showing the comparative prevalence of stone in the bladder in the various occupations followed by those operated on:—

No.	Occupation	Cured	Relieved	Dead
19	Farmers....	17	..	2
3	Gardeners..	3	..	..
3	Weavers....	3	..	..
3	Shoemakers.	3	..	..
1	Quarrier ...	1	..	..
1	Mason	..	1	..
1	Labourer ..	1	..	..
1	Carpenter ..	1	..	..
1	Seaman	1	..	..
1	Boatman ...	1	..	..
1	Fireman....	1	..	..
1	Bleacher ...	1	..	..
1	Widow	1	..	..
1	Schoolboy ..	1	..	..
1	Infant	..	..	1
39	Total,	35	1	3."

—*Edin. Med. and Surg. Journal*, Jan.

REMEDIES FOR CHILBLAINS.

WHILE these remain unbroken, a German practitioner advises the application of the following saponaceous mixture:—Dissolve a drachm of camphor in three drachms of tincture of benzoin, to which add two drachms of iodide of potassium, four drachms of Goulard's extract, an ounce of fresh solution of caustic potass, and two ounces of oil of sweet almonds. These materials are to be mixed in a marble mortar, and, when cold, to be rubbed on the chilblains. (*Note.* In applying this remedy the hands should be kept wetted, and afterwards covered with gloves.) To broken chilblains the following embrocation may be applied, being laid on with a feather, night and morning. ℞ Oil of sweet almonds; lime-water, an ounce each; tinctures of opium and benzoin, two drachms each. Mix. —*Jahrbuch der Prakt. Pharm.*

In Italy a favourite remedy for chilblains is the *pommade di Giacomini*, which is composed of a drachm of acetate of lead, and a drachm of watery extract (*hydrolat cohobé*) of cherry-laurel, to half an ounce of fresh lard. This ointment is lightly smeared over the part affected, and is employed also against other inflammatory eruptions and other skin-diseases. In cases of prurigo about two scruples of tar and half a drachm of camphor are added to the above pomade. —*Revue Scientifique.*

FACE-ACH.—This common affection, so often supposed to be excited by a diseased tooth, although the latter fails to be detected, —a rheumatic, chronic kind of pain, wholly different from that of tic-douloureux,—is often speedily curable by *muriate of ammonia*. This salt should be given in doses of half a drachm, dissolved in water, three or four times daily. About four doses will be sufficient to test the potency of the remedy. At other times the iodide of potassium, in five or six-grain doses, is quickly effective towards a cure. The efficiency of the latter remedy renders it probable that the affection is of the nature of periosteal inflammation.—*Dr. Watson's Lectures.*

THEORY OF HERMAPHRODITISM. — Dr. KNOX considers that as in vertebrate animals the organs of both bronchial and pulmonary respiration are found uniformly to co-exist at an early period, either kind becoming subsequently developed at the expense of the other, so as to the generative organs the type is originally double, the organs of both sexes co-existing in the early stages of development, but one set alone in general developing afterwards at the expense of the antagonist set.

SIGN OF PREGNANCY.—The surest token of pregnancy, says Dr. d'Outrepont, is a *dark bluish hue* of the vaginal mucous membrane (*zunächst gelegenen scheidepartie*), a symptom which is the more valuable inasmuch as it is not evanescent, or confined to one period, but continues from the period of conception to that of labour. Dr. d'Outrepont has not only met with this appearance uniformly in the human subject, but in different animals which he examined in every period of pregnancy, and which he destroyed to ascertain that no fallacy attended the suspicion of gestation.—*Neue Zeitschr. fur Geburtsh.*, xiv., 3.

NEWS OF THE WEEK.

THE mortality of the metropolis in the week ending Dec. 26, 1843, was about stationary with reference to the week preceding, the registered deaths having been 968 (*vice* 967). There was an increase in deaths from diseases of the respiratory organs, viz., 329

(*vice* 300), and from diseases of the digestive organs, viz., 71 (*v.* 58); but a decrease in those from diseases of the nervous system, 149 (*v.* 159), and in those from natural decay, 55 (*v.* 71). *Registered* deaths from diseases of the heart, &c., 21; of the kidneys, &c., 6; in childbed, &c., 15; from organic diseases of uncertain seat, 92; from violence, 24; and from epidemic diseases, 192. Deaths from typhus, measles, and hooping-cough, had diminished, as compared with the week previous, they having been, respectively, 25, 29, and 39 (*vice* 29, 38, and 40); but those from croup, small-pox, and scarlatina, had increased, respectively, to 15, 11, and 49 (from 11, 10, and 46).

ST. GEORGE'S HOSPITAL.—We have been requested (probably by Mr. C. Hawkins) to insert a printed note containing an assurance from Mr. Hawkins (dated Court-yard, Albany, Dec. 28, 1843) that at the late contest for the office of assistant-surgeon to St. George's Hospital he "*was not supported, directly or indirectly, by Sir Benjamin Brodie, who neither voted nor took any part whatever in the election.*" We should observe that the name of Sir Benjamin was never mentioned in this Journal in connection with the proceedings of Mr. Hawkins, or those of his supporters at the election; for we had good reason for believing that the worthy baronet was not taking any part in the struggle.

ROYAL COLLEGE OF SURGEONS OF ENGLAND.

LIST of gentlemen admitted members on Friday, Dec. 29, 1843:—T. W. Houchen, T. Coulson, B. Baker, A. Gosden, B. Kemp, J. B. Bridick, J. H. Brown, E. Wadams, H. L. Hogg, T. T. Chadwick, H. J. Stokes, W. Crofton.

APOTHECARIES' HALL, LONDON.

NAMES of gentlemen who received certificates of qualification to practise as apothecaries on Thursday, Dec. 28, 1843:—George Keane, Ongar, Essex; Kirkham Burton, Wainflete, Lincoln; William Edward Gilchrist Wilson, Leeds.

CORRESPONDENTS.

Sir,—Is there any law relating to lunatic asylums which restrains the resident surgeon of a private institution from engaging in general practice? Your obedient servant,

CHIRURGUS.

*** There is none, of any kind, unless he have made it himself, in his contract with the proprietor.

The cases of *Mr. Corfe* and *Mr. Tuson* shall be inserted next week.

To the Editor.—Sir: Having heard of an extraordinary and interesting case of midwifery that occurred about a month ago in the immediate neighbourhood of Bradford, Yorkshire, in the hands of three of the surgeons of that town, I should desire, through the medium of your most valuable pages, that those gentlemen should be requested to favour their professional brethren with the particulars of the case, which will oblige, Sir, your obedient servant,

AN OLD PRACTITIONER.

Barnsley, Dec. 26th.

*** We insert this note, but if the case occurred in private practice we do not see how the surgeons can be expected to comply with the wish of our correspondent, unless the case had subsequently become the subject of a public judicial inquiry.

Investigator.—The discussion might easily degenerate into a vicious debate. The best mode of preventing that evil will be, in future, to confine it to communications that can be publicly authenticated with the names of the writers, all of whom must be members of the medical profession. We have formed a determination on this subject since publishing the letters inserted in the past and present weeks. The names and addresses of the correspondents, however (in both cases those of medical practitioners), were handed with them to the Editor. We have received many letters besides that of *Investigator*, to which our remarks have reference.

The case forwarded by *Dr. Hutchinson*, in which what is called the hydropathic treatment was adopted, with a fatal result, shall be inserted next week.

The paper of *Mr. Ross*, containing the first part of an account of his chemico-physiological inquiries into the processes of digestion and nutrition, shall be published in an early number.

We shall endeavour to find room for the letter of *Mr. Hytche*, dated Jan. 4th.

R. C. could not act as an apothecary during the interval without being *liable* to the prosecution, which, however, if his intentions have been declared in good faith, probably would not be instituted against him, judging from some cases which have preceded his own.

M. D.—If the party in question forwards communications in which there is no evident effort to praise, glorify, and enrich himself, we shall insert them, but not otherwise. We thank *M. D.* for the kind expressions at the close of his note.

R. F.—Sir Benjamin Brodie is in no respect responsible for the accuracy of the reports of his lectures in this Journal. We shall say a word or two relative to the publication of these excellent practical lectures in the next LANCET.

Mr. Pearson, late a patient in Guy's Hospital, shall receive a private note.

ERRATUM.—In Dr. Bennet's paper on Paracentesis Thoracis, page 444, first line of second paragraph, for 76 read 26.